PHILOSOPHICAL

TRANSACTIONS

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCCXXXVIII.

PART I.

LONDON:

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MDCCCXXXVIII.

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ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the Council-books and Journals of the Society, as from repeated declarations which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries till the Forty-seventh Volume; the Society, as a Body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued, for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable that a Committee of their members should be appointed, to reconsider the papers read before them, and select out of them such as they should judge most proper for publication in the future *Transactions*; which was accordingly done upon the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgement of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a Body,

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upon any subject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently proposed from the Chair, to be given to the authors of such papers as are read at their accustomed meetings, or to the persons through whose hands they received them, are to be considered in no other light than as a matter of civility, in return for the respect shown to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report and even to certify in the public newspapers, that they have met with the highest applause and approbation. And therefore it is hoped that no regard will hereafter be paid to such reports and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

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ROYAL MEDALS.

HER MAJESTY QUEEN VICTORIA, in restoring the Foundation of the Royal Medals, has been graciously pleased to approve of the following regulations for the award of them:

That the Royal Medals be given for such papers only as have been presented to the Royal Society, and inserted in their Transactions.

That the triennial Cycle of subjects be the same as that hitherto in operation: viz.

1. Astronomy; Physiology, including the Natural History of Organized Beings.
2. Physics; Geology and Mineralogy.
3. Mathematics; Chemistry.

That, in case no paper, coming within these stipulations, should be considered deserving of the Royal Medal, in any given year, the Council have the power of awarding such Medal to the author of any other paper on either of the several subjects forming the Cycle, that may have been presented to the Society and inserted in their Transactions; preference being given to the subjects of the year immediately preceding: the award being, in such case, subject to the approbation of Her Majesty.

The Council propose to give one of the Royal Medals in the present year (1838) for the most important unpublished paper in Mathematics, communicated to the Royal Society for insertion in their Transactions after the termination of the Session in June 1835, and prior to the termination of the present Session (June 1838).

The Council propose also to give one of the Royal Medals in the present year (1838)

for the most important unpublished paper in Chemistry, communicated to the Royal Society for insertion in their Transactions after the termination of the Session in June 1835, and prior to the termination of the present Session (June 1838).

The Council propose to give one of the Royal Medals in the year 1839 for the most important unpublished paper in Astronomy, communicated to the Royal Society for insertion in their Transactions after the termination of the Session in June 1836, and prior to the termination of the Session in June 1839.

The Council propose to give also one of the Royal Medals in the year 1839 for the most important unpublished paper in Physiology, including the Natural History of Organized Beings, communicated to the Royal Society for insertion in their Transactions after the termination of the Session in June 1836, and prior to the termination of the Session in June 1839.

The Council propose to give one of the Royal Medals in the year 1840 for the most important unpublished paper in Physics, communicated to the Royal Society for insertion in their Transactions after the termination of the Session in June 1837, and prior to the termination of the Session in June 1840.

The Council propose also to give one of the Royal Medals in the year 1840 for the most important unpublished paper in Geology or Mineralogy, communicated to the Royal Society for insertion in their Transactions after the termination of the Session in June 1837, and prior to the termination of the Session in June 1840.

The Council propose to give one of the Royal Medals in the year 1841 for the most important unpublished paper in Mathematics, communicated to the Royal Society for insertion in their Transactions after the termination of the Session in June 1838, and prior to the termination of the Session in June 1841.

The Council propose also to give one of the Royal Medals in the year 1841 for the most important unpublished paper in Chemistry, communicated to the Royal Society for insertion in their Transactions after the termination of the Session in June 1838, and prior to the termination of the Session in June 1841.

Adjudication of the Medals of the Royal Society for the year 1837 by His Royal Highness the President and Council.

A Copley Medal to M. Becquerel, for his various Memoirs on the subject of Electricity, published in the "Mémoires de l'Académie Royale des Sciences de l'Institut de France," and particularly those on the Production of Crystals of Metallic Sulphurets and Sulphur, by the long-continued action of Electricity of very low tension, and published in the tenth volume of those "Mémoires."

Another Copley Medal to John Frederick Daniell, Esq., F.R.S., for his two papers "On Voltaic Combinations," published in the Philosophical Transactions for 1836.

The Royal Medal, in the department of Physics, to the Rev. William Whewell, M.A., F.R.S., for his "Researches connected with the Theory of the Tides," communicated to the Royal Society, and published in its Transactions within the three preceding years.

The Royal Medal, in the department of Geology, for the year 1837, was not awarded.

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PHILOSOPHICAL TRANSACTIONS.

I. *Experimental Researches in Electricity.—Eleventh Series.* By MICHAEL FARADAY, *Esq.*, *D.C.L. F.R.S. Fullerian Prof. Chem. Royal Institution, Corr. Memb. Royal and Imp. Acadd. of Sciences, Paris, Petersburgh, Florence, Copenhagen, Berlin, &c. &c.*

Received November 30,—Read December 21, 1837.

§. 18. *On Induction.* ¶ i. *Induction an action of contiguous particles.* ¶ ii. *Absolute charge of matter.* ¶ iii. *Electrometer and inductive apparatus employed.* ¶ iv. *Induction in curved lines.* ¶ v. *Specific inductive capacity.* ¶ vi. *General results as to induction.*

¶ i. *Induction an action of contiguous particles.*

1161. THE science of electricity is in that state in which every part of it requires experimental investigation; not merely for the discovery of new effects, but, what is just now of far more importance, the development of the means by which the old effects are produced, and the consequent more accurate determination of the first principles of action of the most extraordinary and universal power in nature:—and to those philosophers who pursue the inquiry zealously yet cautiously, combining experiment with analogy, suspicious of their preconceived notions, paying more respect to a fact than a theory, not too hasty to generalize, and above all things, willing at every step to cross-examine their own opinions, both by reasoning and experiment, no branch of knowledge can afford so fine and ready a field for discovery as this. Such is most abundantly shown to be the case by the progress which electricity has made in the last thirty years: Chemistry and Magnetism have successively acknowledged its over-ruling influence; and it is probable that every effect depending upon the powers of inorganic matter, and perhaps most of those related to vegetable and animal life, will ultimately be found subordinate to it.

1162. Amongst the actions of different kinds into which electricity has conventionally been subdivided, there is, I think, none which excels, or even equals in importance that called *Induction*. It is of the most general influence in electrical phenomena, appearing to be concerned in every one of them, and has in reality the

character of a first, essential, and fundamental principle. Its comprehension is so important, that I think we cannot proceed much further in the investigation of the laws of electricity without a more thorough understanding of its nature; how otherwise can we hope to comprehend the harmony and even unity of action which doubtless governs electrical excitement by friction, by chemical means, by heat, by magnetic influence, by evaporation, and even by the living being?

1163. In the long-continued course of experimental inquiry in which I have been engaged, this general result has pressed upon me constantly, namely, the necessity of admitting two forces, or two forms or directions of a force (516. 517.), combined with the impossibility of separating these two forces (or electricities) from each other, either in the phenomena of statical electricity or those of the current. In association with this, the impossibility under any circumstances, as yet, of absolutely charging matter of any kind with one or the other electricity dwelt on my mind, and made me wish and search for a clearer view than any that I was acquainted with, of the way in which electrical powers and the particles of matter are related; especially in inductive actions, upon which almost all others appeared to rest.

1164. When I discovered the general fact that electrolytes refused to yield their elements to a current when in the solid state, though they gave them forth freely if in the liquid condition (380. 394. 402.), I thought I saw an opening to the elucidation of inductive action, and the possible subjugation of many dissimilar phenomena to one law. For let the electrolyte be water, a plate of ice being coated with platina foil on its two surfaces, and these coatings connected with any continued source of the two electrical powers, the ice will charge like a Leyden arrangement, presenting a case of common induction, but no current will pass. If the ice be liquified, the induction will fall to a certain degree, because a current can now pass; but its passing is dependent upon a *peculiar molecular arrangement* of the particles consistent with the transfer of the elements of the electrolyte in opposite directions, the degree of discharge and the quantity of elements evolved being exactly proportioned to each other (377. 783.). Whether the charging of the metallic coating be effected by a powerful electrical machine, a strong and large voltaic battery, or a single pair of plates, makes no difference in the principle, but only in the degree of action (360.). Common induction takes place in each case if the electrolyte be solid, or if fluid chemical action and decomposition ensue, provided opposing actions do not interfere; and it is of high importance occasionally thus to compare effects in their extreme degrees, for the purpose of enabling us to comprehend the nature of an action in its weak state, which may be only sufficiently evident to us in its stronger condition. As, therefore, in the electrolyte, *induction* appeared to be the *first* step, and *decomposition* the *second* (the power of separating these steps from each other by giving the solid or fluid condition being in our hands); as the induction was the same in its nature as that through air, glass, wax, &c. produced by any of the ordinary means; and as the whole effect in the electrolyte appeared to be an action of the particles thrown into

a peculiar or polarized state, I was led to suspect that common induction itself was in all cases an *action of contiguous particles*, and that electrical action at a distance (i. e. ordinary inductive action) never occurred except through the intermediate influence of the intervening matter.

1165. The respect which I entertain towards the names of EPINUS, CAVENDISH, POISSON, and other most eminent men, all of whose theories I believe consider induction as an action at a distance and in straight lines, long indisposed me to the view I have just stated; and though I always watched for opportunities to prove the opposite opinion, and made such experiments occasionally as seemed to bear directly on the point, as, for instance, the examination of electrolytes, solid and fluid, whilst under induction by polarized light (951. 955.), it is only of late, and by degrees, that the extreme generality of the subject has urged me still further to extend my experiments and publish my view. At present I believe ordinary induction in all cases to be an action of contiguous particles, consisting in a species of polarity, instead of being an action of either particles or masses at sensible distances; and if this be true, the distinction and establishment of such a truth must be of the greatest consequence to our further progress in the investigation of the nature of electric forces. The linked condition of electrical induction with chemical decomposition; of voltaic excitement with chemical action; the transfer of elements in an electrolyte; the original cause of excitement in all cases; the nature and relation of conduction and insulation; of the direct and lateral or transverse action constituting electricity and magnetism; with many other things more or less incomprehensible at present, would all be affected by it, and perhaps receive a full explication in their reduction under one general law.

1166. I searched for an unexceptionable test of my view, not merely in the accordance of known facts with it, but in the consequences which would flow from it if true; especially in those which would not be consistent with the theory of action at a distance. Such a consequence seemed to me to present itself in the direction in which inductive action could be exerted. If in straight lines only, though not perhaps decisive, it would be against my view; if in curved lines also, that would be a natural result of the action of contiguous particles, but I think utterly incompatible with action at a distance, as assumed by the received theories, which, according to every fact and analogy we are acquainted with, is always in straight lines.

1167. Again, if induction be an action of contiguous particles, and also the first step in the process of electrolyzation (1164. 949.), there seemed reason to expect some particular relation of it to the different kinds of matter through which it would be exerted, or something equivalent to a specific electric induction for different bodies, which, if it existed, would unequivocally prove the dependence of induction on the particles; and though this, in the theory of POISSON and others, has never been supposed to be the case, I was soon led to doubt the received opinion, and have taken great pains in subjecting this matter to close experimental examination.

1168. Another ever present question on my mind has been, whether electricity has

an actual and independent existence as a fluid or fluids, or was a mere power of matter, like what we conceive of the attraction of gravitation. If determined either way it would be an enormous advance in our knowledge; and as having the most direct and influential bearing on my notions, I have always sought for experiments which would in any way tend to elucidate that great question. It was in attempts to prove the existence of electricity separate from matter, by giving an independent charge of either positive or negative power to some substance, and the utter failure of all such attempts, whatever substance was used or whatever means of exciting or *evolving* electricity were employed, that first drove me to look upon induction as an action of the particles of matter, each having *both* forces developed in it in exactly equal amount. It is this circumstance, in connection with others, which makes me desirous of placing the remarks on absolute charge first, in the order of proof and argument, which I am about to adduce in favour of my view, that electric induction is an action of the contiguous particles of the insulating medium or *di-electric*.

¶ ii. *On the absolute charge of matter.*

1169. Can matter, either conducting or non-conducting, be charged with one electric force independently of the other, in the least degree, either in a sensible or latent state?

1170. The beautiful experiments of Coulomb upon the equality of action of *conductors*, whatever their substance, and the residence of *all* the electricity upon their surfaces*, are sufficient, if properly viewed, to prove that *conductors cannot be bodily charged*; and as yet no means of communicating electricity to a conductor so as to relate its particles to one electricity, and not at the same time to the other in exactly equal amount, has been discovered.

1171. With regard to electrics or non-conductors, the conclusion does not at first seem so clear. They may easily be electrified bodily, either by communication (1247.) or excitement; but being so charged, every case in succession, when examined, came out to be a case of induction, and not of absolute charge. Thus, glass within conductors could easily have parts not in contact with the conductor brought into an excited state; but it was always found that a portion of the inner surface of the conductor was in an opposite and equivalent state, or that another part of the glass itself was in an equally opposite state, an *inductive* charge and not an *absolute* charge having been acquired.

1172. Well-purified oil of turpentine, which I find to be an excellent liquid insulator for most purposes, was put into a metallic vessel, and, being insulated, was charged, sometimes by contact of the metal with the electrical machine, and at others by a wire dipping into the fluid within; but whatever the mode of communication, no electricity of one kind was retained by the arrangement, except what appeared on the exterior surface of the metal, that portion being there only by an inductive action through the air around. When the oil of turpentine was confined in glass

* Memoires de l'Academie, 1786, pp. 67. 69. 72; 1787, p. 452.

vessels, there were at first some appearances as if the fluid did receive an absolute charge of electricity from the charging wire, but these were quickly reduced to cases of common induction jointly through the fluid, the glass, and the surrounding air.

1173. I carried these experiments on with air to a very great extent. I had a chamber built, being a cube of twelve feet in the side. A slight cubical wooden frame was constructed, and copper wire passed along and across it in various directions, so as to make the sides a large net-work, and then all was covered in with paper, placed in close connection with the wires, and supplied in every direction with bands of tin foil, that the whole might be brought into good metallic communication, and rendered a free conductor in every part. This chamber was insulated in the lecture-room of the Royal Institution; a glass tube above six feet in length was passed through its side, leaving about four feet within and two feet on the outside, and through this a wire passed from the large electrical machine (290.) to the air within. By working the machine, the air within this chamber could be brought into what is considered a highly electrified state (being, in fact, the same state as that of the air of a room in which a powerful machine is in operation), and at the same time the outside of the insulated cube was everywhere strongly charged. But putting the chamber in communication with the perfect discharging train described in a former series (292.), and working the machine so as to bring the air within to its utmost degree of charge, if I quickly cut off the connexion with the machine, and at the same moment or instantly after insulated the cube, the air within had not the least power to communicate a further charge to it. If any portion of the air was electrified, as glass or other insulators may be charged (1171.), it was accompanied by a corresponding opposite action *within* the cube, the whole effect being merely a case of induction. Every attempt to charge air bodily and independently with the least portion of either electricity failed.

1174. I put a delicate gold-leaf electrometer within the cube, and then charged the whole by an *outside* communication, very strongly, for some time together; but neither during the charge or after the discharge did the electrometer or air within show the least signs of electricity. I charged and discharged the whole arrangement in various ways, but in no case could I obtain the least indication of an absolute charge; or of one by induction in which the electricity of one kind had the smallest superiority in quantity over the other. I went into the cube and lived in it, and using lighted candles, electrometers, and all other tests of electrical states, I could not find the least influence upon them, or indication of anything particular given by them, though all the time the outside of the cube was powerfully charged, and large sparks and brushes were darting off from every part of its outer surface. The conclusion I have come to is, that non-conductors, as well as conductors, have never yet had an absolute and independent charge of one electricity communicated to them, and that to all appearance such a state of matter is impossible.

1175. There is another view of this question which may be taken under the sup-

position of the existence of an electric fluid or fluids. It may be impossible to have the one fluid or state in a free condition without its producing by induction the other, and yet possible to have cases in which an isolated portion of matter in one condition being uncharged, shall, by a change of state, evolve one electricity or the other: and though such evolved electricity might immediately induce the opposite state in its neighbourhood, yet the mere evolution of one electricity without the other in the *first instance*, would be a very important fact in the theory which assumes a fluid or fluids: these theories as I understand them assigning not the slightest reason why such an effect should not occur.

1176. But on searching for such cases I cannot find one. Evolution by friction, as is well known, gives both powers in equal proportion. So does evolution by chemical action, notwithstanding the great diversity of bodies which may be employed, and the enormous quantity of electricity which can in this manner be evolved (371. 376. 861. 868.) The more promising cases of change of state, whether by evaporation, fusion, or the reverse processes, still give both forms of the power in *equal* proportion; and the cases of splitting of mica and other crystals, the breaking of sulphur, &c. &c., are subject to the same limitation.

1177. As far as experiment has proceeded, it appears, therefore, impossible either to evolve or make disappear one electric force without equal and corresponding change in the other. It is also equally impossible experimentally to charge a portion of matter with one electric force independently of the other. Charge always implies *induction*, for it can in no instance be effected without; and also the presence of the *two* forms of power, equally at the moment of development and afterwards. There is no *absolute* charge of matter with one fluid; no latency of a single electricity. This though a negative result is an exceedingly important one, being probably the consequence of a natural impossibility, which will become clear to us when we understand the true condition and theory of the electric power.

1178. The preceding considerations already point to the following conclusions: bodies cannot be charged absolutely, but only relatively, and by a principle which is the same with that of *induction*. All *charge* is sustained by induction. All phenomena of *intensity* include the principle of induction. All *excitation* is dependent on or directly related to induction. All *currents* involve previous intensity and therefore previous induction. INDUCTION appears to be the essential function both in the first development and the consequent phenomena of electricity.

¶ iii. *Electrometer and inductive apparatus employed.*

1179. Leaving for a time the further consideration of the preceding facts until they can be collated with other results bearing directly on the great question of the nature of induction, I will now describe the apparatus I have had occasion to use; and in proportion to the importance of the principles sought to be established is the necessity of doing this so clearly, as to leave no doubt of the results behind.

1180. *Electrometer.* The measuring instrument I have employed has been the torsion balance electrometer of COULOMB, constructed, generally, according to his instructions*, but with certain variations and additions, which I will briefly describe. The lower part was a glass cylinder eight inches in height and eight inches in diameter; the tube for the torsion thread was seventeen inches in length. The torsion thread itself was not of metal, but glass, according to the excellent suggestion of the late Dr. RITCHIE†. It was twenty inches in length, and of such tenuity that when the shell lac lever and attached ball, &c. were connected with it, they made about ten vibrations in a minute. It would bear torsion through four revolutions or 1440°, and yet, when released, return accurately to its position; probably it would have borne considerably more than this without injury. The repelled ball was of pith, gilt, and was 0·3 of an inch in diameter. The horizontal stem or lever supporting it was of shell lac, according to COULOMB's direction, the arm carrying the ball being 2·4 inches long, and the other only 1·2 inches: to this was attached the vane, also described by COULOMB, which I found to answer admirably its purpose of quickly destroying vibrations. That the inductive action within the electrometer might be uniform in all positions of the repelled ball and in all states of the apparatus, two bands of tin foil, about an inch wide each, were attached to the inner surface of the glass cylinder, going entirely round it, at the distance of 0·4 of an inch from each other, and at such a height that the intermediate clear surface was in the same horizontal plane with the lever and ball. These bands were connected with each other and with the earth, and, being perfect conductors, always exerted a uniform influence on the electrified balls within, which the glass surface, from its irregularity of condition at different times, I found, did not. For the purpose of keeping the air within the electrometer in a constant state as to dryness, a glass dish, of such size as to enter easily within the cylinder, had a layer of fused potash placed within it, and this being covered with a disc of fine wire gauze to render its inductive action uniform at all parts, was placed within the instrument at the bottom and left there.

1181. The moveable ball used to take and measure the portion of electricity under examination, and which may be called the *repelling*, or the *carrier*, ball, was of soft alder wood, well and smoothly gilt. It was attached to a fine shell lac stem, and introduced through a hole into the electrometer according to COULOMB's method: the stem was fixed at its upper end in a block or vice, supported on three short feet; and on the surface of the glass cover above was a plate of lead with stops on it, so that when the carrier ball was adjusted in its right position, with the vice above bearing at the same time against these stops, it was perfectly easy to bring away the carrier ball and restore it to its place again very accurately, without any loss of time.

1182. It is quite necessary to attend to certain precautions respecting these balls. If of pith alone they are bad; for when very dry, that substance is so imperfect a

* Memoires de l'Academie, 1785, p. 570.

† Philosophical Transactions, 1830.

conductor that it neither receives nor gives a charge freely, and so, after contact with a charged conductor, is liable to be in an uncertain condition. Again, it is difficult to turn pith so smoothly as to leave the ball, even when gilt, sufficiently free from irregularities of form, as to retain its charge undiminished for a considerable length of time. When, therefore, the balls are finally prepared and gilt they should be examined, and being electrified, unless they can hold their charge with very little diminution for a considerable time, and yet be discharged instantly and perfectly by the touch of an uninsulated conductor, they should be dismissed.

1183. It is, perhaps, unnecessary to refer to the graduation of the instrument, further than to explain how the observations were made. On a circle or ring of paper on the outside of the glass cylinder, fixed so as to cover the internal lower ring of tin foil, were marked four points corresponding to angles of 90°; four other points exactly corresponding to these points being marked on the upper ring of tin foil within. By these and the adjusting screws on which the whole instrument stands, the glass torsion thread could be brought accurately into the centre of the instrument and of the graduations on it. From one of the four points on the exterior of the cylinder a graduation of 90° was set off, and a corresponding graduation was placed upon the upper tin foil on the opposite side of the cylinder within; and a dot being marked on that point of the surface of the repelled ball nearest to the side of the electrometer, it was easy, by observing the line which this dot made with the lines of the two graduations just referred to, to ascertain accurately the position of the ball. The upper end of the glass thread was attached, as in COULOMB'S original electrometer, to an index, which had its appropriate graduated circle, upon which the degree of torsion was ultimately to be read off.

1184. After the levelling of the instrument and adjustment of the glass thread, the blocks which determine the place of the *carrier ball* are to be regulated (1181.) so that, when the carrier arrangement is placed against them, the centre of the ball may be in the radius of the instrument corresponding to 0° on the lower graduation or that on the side of the electrometer, and at the same level and distance from the centre as the *repelled ball* on the suspended torsion lever. Then the torsion index is to be turned until the ball connected with it (the repelled ball) is accurately at 30°, and finally the graduated arch belonging to the torsion index is to be adjusted so as to bring 0° upon it to the index. This state of the instrument was adopted as that which gave the most direct expression of the experimental results, and in the form having fewest variable errors; the angular distance of 30° being always retained as the standard distance to which the balls were in every case to be brought, and the whole of the torsion being read off at once on the graduated circle above. Under these circumstances the distance of the balls from each other was not merely the same in degree, but their position in the instrument, and in relation to every part of it, was actually the same every time that a measurement was made; so that all irregularities arising from slight difference of form and action in the instrument and the

bodies around were avoided. The only difference which could occur in the position of anything within, consisted in the deflexion of the torsion thread from a vertical position, more or less, according to the force of repulsion of the balls; but this was so slight as to cause no interfering difference in the symmetry of form within the instrument, and gave no error in the amount of torsion force indicated on the graduation above.

1185. Although the constant angular distance of 30° between the centres of the balls was adopted, and found abundantly sensible, for all ordinary purposes, yet the facility of rendering the instrument far more sensible by diminishing this distance was at perfect command; the results at different distances being very easily compared with each other either by experiment, or, as they are inversely as the squares of the distances, by calculation.

1186. The COULOMB balance electrometer requires experience to be understood; but I think it a very valuable instrument in the hands of those who will take pains by practice and attention to learn the precautions needful in its use. Its insulating condition varies with circumstances, and should be examined before it is employed in experiments. In an ordinary and fair condition, when the balls were so electrified as to give a repulsive torsion force of 400° at the standard distance of 30°, it took nearly four hours to sink to 50° at the same distance; the average loss from 400° to 300° being at the rate of 2°·7 per minute, from 300° to 200° of 1°·7 per minute, from 200° to 100° of 1°·3 per minute, and from 100° to 50° of 0°·87 per minute. As a complete measurement by the instrument may be made in much less than a minute, the amount of loss in that time is but small, and can easily be taken into account.

1187. *The inductive apparatus.*—My object was to examine inductive action carefully when taking place through different media, for which purpose it was necessary to subject these media to it in exactly similar circumstances, and in such quantities as should suffice to eliminate any variations they might present. The requisites of the apparatus to be constructed were, therefore, that the inducing surfaces of the conductors should have a constant form and state, and be at a constant distance from each other; and that either solids, fluids, or gases might be placed and retained between these surfaces with readiness and certainty, and for any length of time.

1188. The apparatus used may be described in general terms as consisting of two metallic spheres of unequal diameter, placed, the smaller within the larger, and concentric with it; the interval between the two being the space through which the induction was to take place. A section of it is given (Plate I. fig. 1.) on a scale of one half: *a*, *a* are the two halves of a brass sphere, with an air-tight joint at *b*, like that of the Magdeburg hemispheres, made perfectly flush and smooth inside so as to present no irregularity; *c* is a connecting piece by which the apparatus is joined to a good stop-cock *d*, which is itself attached either to the metallic foot *e*, or to an air pump. The aperture within the hemisphere at *f* is very small: *g* is a brass collar fitted to the upper hemisphere, through which the shell lac support of the inner ball

and its stem passes; *h* is the inner ball, also of brass; it screws on to a brass stem *i*, terminated above by a brass ball B; *l*, *l* is a mass of shell lac, moulded carefully on to *i*, and serving both to support and insulate it and its balls *h*, B. The shell-lac stem *l* is fitted into the socket *g*, by a little ordinary resinous cement, more fusible than shell-lac, applied at *m m* in such a way as to give sufficient strength and render the apparatus air-tight there, yet leave as much as possible of the lower part of the shell-lac stem untouched, as an insulation between the ball *h* and the surrounding sphere *a*, *a*. The ball *h* has a small aperture at *n*, so that when the apparatus is exhausted of one gas and filled with another, the ball *h* may itself also be exhausted and filled, that no variation of the gas in the interval *o* may occur during the course of an experiment.

1189. It will be unnecessary to give the dimensions of all the parts, since the drawing is to a scale of one half: the inner ball has a diameter of 2·33 inches, and the surrounding sphere an internal diameter of 3·57 inches. Hence the width of the intervening space, through which the induction is to take place, is 0·62 of an inch; and the extent of this place or plate, i. e. the surface of a medium sphere, may be taken as twenty-seven square inches, a quantity considered as sufficiently large for the comparison of different substances. Great care was taken in finishing well the inducing surfaces of the ball *h* and sphere *a*, *a*; and no varnish or lacquer was applied to them, or to any part of the metal of the apparatus.

1190. The attachment and adjustment of the shell-lac stem was a matter requiring considerable care, especially as, in consequence of its cracking, it had frequently to be renewed. The best lac was chosen and applied to the wire *i*, so as to be in good contact with it everywhere, and in perfect continuity throughout its own mass. It was not thinner than is given by scale in the drawing, for when less it frequently cracked within a few hours after its cooling. I think that very slow cooling or annealing improved its quality in this respect. The collar *g* was made as thin as could be, that the lac might be as large there as possible. In order that at every re-attachment of the stem to the upper hemisphere the ball *h* might have the same relative position, a gauge *p* (fig. 2.) was made of wood, and this being applied to the ball and hemisphere whilst the cement at *m* was still soft, the bearings of the ball at *q q*, and the hemisphere at *r r*, were forced home, and the whole left until cold. Thus all difficulty in the adjustment of the ball in the sphere was avoided.

1191. I had occasion at first to attach the stem to the socket by other means, as a band of paper or a plugging of white silk thread; but these were very inferior to the cement, interfering much with the insulating power of the apparatus.

1192. The retentive power of this apparatus was, when in good condition, better than that of the electrometer (1186.), i. e. the proportion of loss of power was less. Thus when the apparatus was electrified, and also the balls in the electrometer, to such a degree, that after the inner ball had been in contact with the top *k* of the ball of the apparatus, it caused a repulsion indicated by 600° of torsion force, then

in falling from 600° to 400° the average loss was 8°·6 per minute; from 400° to 300° the average loss was 2°·6 per minute; from 300° to 200° it was 1°·7 per minute; from 200° to 170° it was 1° per minute. This was after the apparatus had been charged for a short time; at the first instant of charging there is an apparent loss of electricity, which can only be comprehended hereafter (1207. 1250.).

1193. When the apparatus loses its insulating power suddenly, it is almost always from a crack near to or within the brass socket. These cracks are usually transverse to the stem. If they occur at the part attached by common cement to the socket, the air cannot enter, and being then as vacua, they conduct away the electricity and lower the charge, as fast almost as if a piece of metal had been introduced there. Occasionally stems in this state, being taken out and cleared from the common cement, may, by the careful application of the heat of a spirit lamp, be so far softened and melted as to renew perfect continuity of the parts; but if that does not succeed in restoring things to a good condition, the remedy is a new shell-lac stem.

1194. The apparatus when in order could easily be exhausted of air and filled with any given gas; but when that gas was acid or alkaline, it could not properly be removed by the air-pump, and yet required to be perfectly cleared away. In such cases the apparatus was opened and cleared; and with respect to the inner ball *h*, it was washed out two or three times with distilled water introduced at the screw hole, and then being heated above 212°, air was blown through to render the interior perfectly dry.

1195. The inductive apparatus described is evidently a Leyden phial, with the advantage, however, of having the dielectric or insulating medium changed at pleasure. The balls *h* and B, with the connecting wire *i*, constitute the charged conductor, upon the surface of which all the electric force is resident by virtue of induction (1178.). Now though the largest portion of this induction is between the ball *h* and the surrounding sphere *a a*, yet the wire *i* and the ball B determine a part of the induction from their surfaces towards the external surrounding conductors. Still, as all things in that respect remain the same, whilst the medium within at *o o*, may be varied, any changes exhibited by the whole apparatus will in such cases depend upon the variations made in the interior; and it was these changes I was in search of, the negation or establishment of such differences being the great object of my inquiry. I considered that these differences, if they existed, would be most distinctly set forth by having two apparatus of the kind described, precisely similar in every respect; and then, different insulating media being within, to charge one and measure it, and after dividing the charge with the other, to observe what the ultimate conditions of both were. If insulating media really had any specific differences in favouring or opposing inductive action through them, such differences, I conceived, could not fail of being developed by such a process.

1196. I will wind up this description of the apparatus, and explain the precautions necessary in their use, by describing the form and order of the experiments made to

prove their equality when both contained common air. In order to facilitate reference I will distinguish the two by the terms App. i. and App. ii.

1197. The electrometer is first to be adjusted and examined (1184.), and the app. i. and ii. are to be perfectly discharged. A Leyden phial is to be charged to such a degree that it would give a spark of about one-sixteenth or one-twentieth of an inch in length between two balls of half an inch diameter; and the carrier ball of the electrometer being charged by this phial, is to be introduced into the electrometer, and the lever ball brought by the motion of the torsion index against it; the charge is thus divided between the balls, and repulsion ensues. It is useful then to bring the repelled ball to the standard distance of 30° by the motion of the torsion index, and observe the force in degrees required for this purpose; this force will in future experiments be called *repulsion of the balls*.

1198. One of the inductive apparatus, as, for instance, app. i., is now to be charged from the Leyden phial, the latter being in the state it was in when used to charge the balls; the carrier ball is to be brought into contact with the top of its upper ball (*k*, fig. 1.), then introduced into the electrometer, and the repulsive force (at the distance of 30°) measured. Again, the carrier should be applied to the app. i. and the measurement repeated; the apparatus i. and ii. are then to be joined, so as to *divide* the charge, and afterwards the force of each measured by the carrier ball, applied as before, and the results carefully noted. After this both i. and ii. are to be discharged; then app. ii. charged, measured, divided with app. i., and the force of each again measured and noted. If in each case the half charges of app. i. and ii. are equal, and are together equal to the whole charge before division, then it may be considered as proved that the two apparatus are precisely equal in power, and fit to be used in cases of comparison between different insulating media or *dielectrics*.

1199. But the *precautions* necessary to obtain accurate results are numerous. The apparatus i. and ii. must always be placed on a thoroughly uninsulating medium. A mahogany table, for instance, is far from satisfactory in this respect, and therefore a sheet of tin foil, connected with an extensive discharging train (292.), is what I have used. They must be so placed also as not to be too near each other, and yet equally exposed to the inductive influence of surrounding objects; and these objects, again, should not be disturbed in their position during an experiment, or else variations of induction upon the external ball B of the apparatus may occur, and so errors be introduced into the results. The carrier ball, when receiving its portion of electricity from the apparatus, should always be applied at the same part of the ball, as, for instance, the summit *k*, and always in the same way; variable induction from the vicinity of the head, hands, &c. being avoided, and the ball after contact being withdrawn upwards in a regular and constant manner.

1200. As the stem had occasionally to be changed (1190.), and the change might occasion slight variations in the position of the ball within, I made such a variation

purposely, to the amount of an eighth of an inch (which is far more than ever could occur in practice), but did not find that it sensibly altered the relation of the apparatus, or its inductive condition *as a whole*. Another trial of the apparatus was made as to the effect of dampness in the air, one being filled with very dry air, and the other with air from over water. Though this produced no change in the result, except an occasional tendency to more rapid dissipation, yet the precaution was always taken when working with gases (1290.) to dry them perfectly.

1201. It is essential that the interior of the apparatus should be *perfectly* free from dust or small loose particles, for these very rapidly lower the charge and interfere on occasions when their presence and action would hardly be expected. To breathe on the interior of the apparatus and wipe it out quietly with a clean silk handkerchief, is an effectual way of removing them; but then the intrusion of other particles should be carefully guarded against, and a dusty atmosphere should for this and several other reasons be avoided.

1202. The shell lac stem requires occasionally to be well wiped, to remove, in the first instance, the film of wax and adhering matter which is upon it; and afterwards to displace dirt and dust which will gradually attach to it in the course of experiments. I have found much to depend upon this precaution, and a silk handkerchief is the best wiper.

1203. But wiping and some other circumstances tend to give a charge to the surface of the shell lac stem. This should be removed, for, if allowed to remain, it very seriously affects the degree of charge given to the carrier ball by the apparatus (1232.). This condition of the stem is best observed by discharging the apparatus, applying the carrier ball to the stem, touching it with the finger, insulating and removing it, and examining whether it has received any charge (by induction) from the stem; if it has, the stem itself is in a charged state. The best method of removing the charge I have found to be, to cover the finger with a single fold of a silk handkerchief, and breathing on the stem, to wipe it immediately after with the finger, the ball B and its connected wire, &c. being at the same time *uninsulated*: the wiping place of the silk must not be changed; it then becomes sufficiently damp not to excite the stem, and is yet dry enough to leave it in a clean and excellent insulating condition. If the air be dusty, it will be found that a single charge of the apparatus will bring on an electric state of the outside of the stem, in consequence of the carrying power of the particles of dust; whereas in the morning, and in a room which has been left quiet, several experiments can be made in succession without the stem assuming the least degree of charge.

1204. Experiments should not be made by candle or lamp light except with much care, for flames have great and yet unsteady powers of affecting and dissipating electrical charges.

1205. As a final observation on the state of the apparatus, they should retain their charge well and uniformly, and alike for both, and at the same time allow of a per-

fect and instantaneous discharge, giving then no charge to the carrier ball, whatever part of the ball B it may be applied to (1218.).

1206. With respect to the balance electrometer all the precautions that need be mentioned, are, that the carrier ball is to be preserved during the first part of an experiment in its electrified state, the loss of electricity which would follow upon its discharge being avoided; and, that in introducing it into the electrometer through the hole in the glass plate above, care should be taken that it do not touch, or even come near to, the edge of the glass.

1207. When the whole charge in one apparatus is divided between the two, the gradual fall, apparently from dissipation, in the apparatus which has *received* the half charge is greater than in the one *originally* charged. This is due to a peculiar effect to be described hereafter (1250. 1251.), the interfering influence of which may be avoided to a great extent by going through the steps of the process regularly and quickly; therefore, after the original charge has been measured, in app. i. for instance, i. and ii. are to be symmetrically joined by their balls B, the carrier touching one of these balls at the same time; it is first to be removed, and then the apparatus separated from each other; app. ii. is next quickly to be measured by the carrier, then app. i.; lastly, ii. is to be discharged, and the discharged carrier applied to it to ascertain whether any residual effect is present (1205.), and app. i. being discharged is also to be examined in the same manner and for the same purpose.

1208. The following is an example of the division of a charge by the two apparatus, air being the dielectric in both of them. The observations are set down one under the other in the order in which they were taken, the left hand numbers representing the observations made on app. i. and the right hand numbers those on app. ii. App. i. is that which was originally charged, and after two measurements, the charge was divided with app. ii.

App. i.	App. ii.
Balls 160°	
. . . .	0°
254°	
250	
divided and instantly taken	
. . . .	122
124	
1	after being discharged.
. . . .	2 after being discharged.

1209. Without endeavouring to allow for the loss which must have been gradually going on during the time of the experiment, let us observe the results of the numbers as they stand. As 1° remained in app. i. in an undischargeable state, 249° may be taken as the utmost amount of the transferable or divisible charge, the half of which is 124°·5. As app. ii. was free of charge in the first instance, and immediately after

the division was found with 122°, this amount *at least* may be taken as what it had received. On the other hand 124° minus 1°, or 123°, may be taken as the half of the transferable charge retained by app. i. Now these do not differ much from each other, or from 124°·5, the half of the full amount of transferable charge; and when the gradual loss of charge evident in the difference between 254° and 250° of app. i. is also taken into account, there is every reason to admit the result as showing an equal division of charge, *unattended by any disappearance of power* except that due to dissipation.

1210. I will give another result, in which app. ii. was first charged, and where the residual action of that apparatus was greater than in the former case.

App. i.	App. ii.	
	Balls 150°	
	. . . . 152°	
	 148	
divided and instantly taken		
70°		
	 78	
	 5	immediately after discharge.
0		immediately after discharge.

1211. The transferable charge being 148°—5°, its half is 71°·5, which is not far removed from 70°, the half charge of i.; or from 73°, the half charge of ii.: these half charges again making up the sum of 143°, or just the amount of the whole transferable charge. Considering the errors of experiment, therefore, these results may again be received as showing that the apparatus were equal in inductive capacity, or in their powers of receiving charges.

1212. The experiments were repeated with charges of negative electricity with the same general results.

1213. That I might be sure of the sensibility and action of the apparatus, I made such a change in one as ought upon principle to increase its inductive force, i. e. I put a metallic lining into the lower hemisphere of app. i., so as to diminish the thickness of the intervening air in that part, from 0·62 to 0·435 of an inch: this lining was carefully shaped and rounded so that it should not present a sudden projection within at its edge, but a gradual transition from the reduced interval in the lower part of the sphere to the larger one in the upper.

1214. This change immediately caused app. i. to produce effects indicating that it had a greater aptness or capacity for induction than app. ii. Thus, when a transferable charge in app. ii. of 469° was divided with app. i., the former retained a charge of 225°, whilst the latter showed one of 227°, i. e. the former had lost 244° in communicating 227° to the latter: on the other hand, when app. i. had a transferable charge in it of 381° divided by contact with app. ii., it lost 181° only, whilst it gave to app. ii. as many as 194°:—the sum of the divided forces being in the first instance

less, and in the second instance *greater* than the original undivided charge. These results are the more striking, as only one half of the interior of app. i. was modified, and they show that the instruments are capable of bringing out differences in inductive force from amongst the errors of experiment, when these differences are much less than that produced by the alteration made in the present instance.

¶ iv. *Induction in curved lines.*

1215. Amongst those results deduced from the molecular view of induction (1166.), which, being of a peculiar nature, are the best tests of the truth or error of the theory, the expected action in curved lines is, I think, the most important at present; for, if shown to take place in an unexceptionable manner, I do not see how the old theory of action at a distance and in straight lines can stand, or how the conclusion that ordinary induction is an action of contiguous particles can be resisted.

1216. There are many forms of old experiments which might be quoted as favourable to, and consistent with the view I have adopted. Such are most cases of electro-chemical decomposition, electrical brushes, auras, sparks, &c.; but as these might be considered equivocal evidence, inasmuch as they include a current and discharge, (though they have long been to me indications of prior molecular action (1230.)) I endeavoured to devise such experiments for first proofs as should not include transfer, but relate altogether to the pure simple inductive action of statical electricity.

1217. It was also of importance to make these experiments in the simplest possible manner, using not more than one insulating medium or dielectric at a time, lest differences of slow conduction should produce effects which might erroneously be supposed to result from induction in curved lines. It will be unnecessary to describe the steps of the investigation minutely; I will at once proceed to the simplest mode of proving the facts, first in air and then in other insulating media.

1218. A cylinder of solid shell-lac, 0·9 of an inch in diameter and seven inches in length, was fixed upright in a wooden foot (fig. 3.): it was made concave or cupped at its upper extremity so that a brass ball or other small arrangement could stand upon it. The upper half of the stem having been excited *negatively* by friction with warm flannel, a brass ball, B, 1 inch in diameter, was placed on the top, and then the whole arrangement examined by the carrier ball and Coulomb's electrometer (1180. &c.). For this purpose the balls of the electrometer were charged *positively* to about 360°, and then the carrier being applied to various parts of the ball B, the two were uninsulated whilst in contact or in position, then insulated*, separated, and the charge of the carrier examined as to its nature and force. Its electricity was always positive, and its force at the different positions *a*, *b*, *c*, *d*, &c. (figs. 3. and 4.) observed in succession, was as follows:

* It can hardly be necessary for me to say here, that whatever general state the carrier ball acquired in any place where it was uninsulated and then insulated, it retained on removal from that place, notwithstanding that it might pass through other places that would have given to it, if uninsulated, a different condition.

at	*a* . .	above 1000°
	b it was . . .	149
	c	270
	d	512
	b	130

1219. To comprehend the full force of these results, it must first be understood, that all the charges of the ball B and the carrier are charges by induction, from the action of the excited surface of the shell-lac cylinder; for whatever electricity the ball B received by *communication* from the shell-lac, either in the first instance or afterwards, was removed by the uninsulating contacts, only that due to induction remaining; and this is shown by the charges taken from the ball in this its uninsulated state being always positive, or of the contrary character to the electricity of the shell-lac. In the next place the charges at *a*, *c*, and *d* were of such a nature as might be expected from an inductive action in straight lines, but that obtained at *b* is *not so*: it is clearly a charge by induction, but *induction* in *a curved line*; for the carrier ball whilst applied to *b*, and after its removal to a distance of six inches or more from B, could not, in consequence of the size of B, be connected by a straight line with any part of the excited and inducing shell-lac.

1220. To suppose that the upper part of the *uninsulated* ball B, should in some way be retained in an electrified state by that portion of the surface which is in sight of the shell-lac, would be in opposition to what we know already of the subject. Electricity is retained upon the surface of conductors only by induction (1178.); and though some persons may not be prepared as yet to admit this with respect to insulated conductors, all will as regards uninsulated conductors like the ball B; and to decide the matter we have only to place the carrier ball at *e* (fig. 4.), so that it shall not come in contact with B, uninsulate it by a metallic rod descending perpendicularly, insulate it, remove it, and examine its state; it will be found charged with the same kind of electricity as, and even to a higher degree (1224.) than, if it had been in contact with the summit of B.

1221. To suppose, again, that induction acts in some way *through or across* the metal of the ball, is negatived by the simplest considerations; but a fact in proof will be better. If instead of the ball B a small disc of metal be used, the carrier may be charged at, or above the middle of its upper surface: but if the plate be enlarged to about $1\frac{1}{2}$ or 2 inches in diameter, C (fig. 5.), then no charge will be given to the carrier at *f*, though when applied nearer to the edge at *g*, or even *above the middle* at *h*, a charge will be obtained; and this is true though the plate may be a mere thin film of gold leaf. Hence it is clear that the induction is not *through* the metal, but through the air or dielectric, and that in curved lines.

1222. I had another arrangement, in which a wire passing downwards through the middle of the shell-lac cylinder to the earth, was connected with the ball B (fig. 6.)

so as to keep it in a constantly uninsulated state. This was a very convenient form of apparatus, and the results with it were the same as those described.

1223. In another case the ball B was supported by a shell-lac stem, independently of the excited cylinder of shell-lac, and at half an inch distance from it; but the effects were the same. Then the brass ball of a charged Leyden jar was used in place of the excited shell-lac to produce induction; but this caused no alteration of the phenomena. Both positive and negative inducing charges were tried with the same general results. Finally, the arrangement was inverted in the air for the purpose of removing every possible objection to the conclusions, but they came out exactly the same.

1224. Some results obtained with a brass hemisphere instead of the ball B were exceedingly interesting. It was 1·36 of an inch in diameter, (fig. 7.), and being placed on the top of the excited shell-lac cylinder, the carrier ball was applied, as in the former experiments (1218.), at the respective positions delineated in the figure. At *i* the force was 112°, at *k* 108°, at *l* 65°, at *m* 35°; the inductive force gradually diminishing, as might have been expected, to this point. But on raising the carrier to the position *n* the charge increased to 87°; and on raising it still higher to *a*, the charge still further increased to 105°: at a higher point still, *p*, the charge taken was smaller in amount, being 98°, and continued to diminish for more elevated positions. Here the induction fairly turned a corner. Nothing, in fact, can better show both the curved lines or courses of the inductive action, disturbed as they are from their rectilineal form by the shape, position, and condition of the metallic hemisphere; and also a *lateral tension*, so to speak, of these lines on one another:—all depending, as I conceive, on induction being an action of the contiguous particles of the dielectric thrown into a state of polarity and tension, and mutually related by their forces in all directions.

1225. As another proof that the whole of these actions were inductive I may state a result which was exactly what might be expected, namely, that if uninsulated conducting matter was brought round and near to the excited shell-lac stem, then the inductive force was directed towards it, and could not be found on the top of the hemisphere. Removing this matter the lines of force resumed their former direction. The experiment affords proofs of the lateral tension of these lines, and supplies a warning to remove such matter in repeating the above investigation.

1226. After these results on curved inductive action in air I extended the experiments to other gases, using first carbonic acid and then hydrogen: the phenomena were precisely those already described. In these experiments I found that if the gases were confined in vessels they required to be very large, for whether of glass or earthenware, the conducting power of such materials is so great that the induction of the excited shell-lac cylinder towards them is as much as if they were metal; and if the vessels be small, so great a portion of the inductive force is determined

towards them that the lateral tension or mutual repulsion of the lines of force before spoken of, (1224.) by which their inflexion is caused, is so much relieved in other directions, that no inductive charge will be given to the carrier ball in the positions *k*, *l*, *m*, *n*, *o*, *p*, (fig. 7.). A very good mode of making the experiment is to let large currents of the gases ascend or descend through the air, and carry on the experiments in these currents.

1227. These experiments were then varied by the substitution of a liquid dielectric, namely, *oil of turpentine*, in place of air and gases. A dish of thin glass well covered with a film of shell-lac, (1272.) and found by trial to insulate well, had some highly rectified oil of turpentine put into it to the depth of half an inch, and being then placed upon the top of the brass hemisphere, (fig. 7.) observations were made with the carrier ball as before (1224.). The results were the same, and the circumstance of some of the positions being within the fluid and some without, made no sensible difference.

1228. Lastly, I used a few solid dielectrics for the same purpose, and with the same results. These were shell-lac, sulphur, fused and cast borate of lead, flint glass well covered with a film of lac, and spermaceti. The following was the form of experiment with sulphur, and all were of the same kind. A square plate of the substance, two inches in extent and 0·6 of an inch in thickness, was cast with a small hole or depression in the middle of one surface to receive the carrier ball. This was placed upon the surface of the metal hemisphere (fig. 9.) arranged on the excited lac as in former cases, and observations were made at *n*, *o*, *p*, and *q*. Great care was required in these experiments to free the sulphur or other solid substance from any charge it might previously have received. This was done by breathing and wiping (1203.), and the substance being found free from all electrical excitement, was then used in the experiment; after which it was removed and again examined, to ascertain that it had received no charge, but had acted really as a dielectric. With all these precautions the results were the same: and it is thus very satisfactory to obtain the curved inductive action through *solid bodies*, as any possible effect from the translation of charged particles in fluids or gases, which some persons might imagine to be the case, is here entirely negatived.

1229. In these experiments with solid dielectrics, the degree of charge assumed by the carrier ball at the situations *n*, *o*, *p* (fig. 9.), was decidedly greater than that given to the ball at the same places when air only intervened between it and the metal hemisphere. This effect is consistent with what will hereafter be found to be the respective relations of these bodies, as to their power of facilitating induction through them (1269. 1273. 1277.).

1230. I might quote *many* other forms of experiment, some old and some new, in which induction in curved or contorted lines takes place, but think it unnecessary after the preceding results; I shall therefore mention but two. If a conductor A, (fig. 8.) be electrified, and an uninsulated metallic ball B, or even a plate, provided the edges be not too thin, be held before it, a small electrometer at *c* or at *d*, uninsu-

lated, will give signs of electricity, opposite in its nature to that of A, and therefore caused by induction, although the influencing and influenced bodies cannot be joined by a right line passing through the air. Or if, the electrometers being removed, a point be fixed at the back of the ball in its uninsulated state as at C, this point will become luminous and discharge the conductor A. The latter experiment is described by NICHOLSON*, who, however, reasons erroneously upon it. As to its introduction here, though it is a case of discharge, the discharge is preceded by induction, and that induction must be in curved lines.

1231. As argument against the received theory of induction and in favour of that which I have ventured to put forth, I cannot see how the preceding results can be avoided. The effects are clearly inductive effects produced by electricity, not in currents but in its statical state, and this induction is exerted in lines of force which, though in many experiments they may be straight, are here curved more or less according to circumstances. I use the term *line of inductive force* merely as a temporary conventional mode of expressing the direction of the power in cases of induction; and in the experiments with the hemisphere (1224.), it is curious to see how, when certain lines have terminated on the under surface and edge of the metal, those which were before lateral to them *expand and open out from each other*, some bending round and terminating their action on the upper surface of the hemisphere, and others meeting, as it were, above in their progress outwards, uniting their forces to give an increased charge in the carrier ball, at an *increased distance* from the source of power, and influencing each other so as to cause a second flexure in the contrary direction from the first one. All this appears to me to prove that the whole action is one of contiguous particles, related to each other, not merely in the lines which they may be conceived to form through the dielectric, between the inductric and the inducteous surfaces, but in other lateral directions also. It is this which gives the effect equivalent to lateral repulsion or expansion in the lines of force I have spoken of, and enables induction to turn a corner (1304.). The power, instead of being like that of gravity, which relates particles together through straight lines, whatever other particles may be between them, is more analogous to that of a series of magnetic needles, or to the condition of the particles considered as forming the whole of a straight or a curved magnet. So that in whatever way I view it, and with great suspicion of the influence of favourite notions over myself, I cannot perceive how the ordinary theory of induction can be a correct representation of that great natural principle of electrical action.

1232. I have had occasion in describing the precautions necessary in the use of the inductive apparatus, to refer to one founded on induction in curved lines (1203.); and after the experiments already described, it will easily be seen how great an influence the shell-lac stem may exert upon the charge of the carrier ball when applied to the apparatus (1218.), unless that precaution be attended to.

* Encyclopædia Britannica, vol. vi. p. 504.

1233. I think it expedient, next in the course of these experimental researches, to describe some effects due to *conduction*, obtained with such bodies as glass, lac, sulphur, &c., which had not been anticipated. Being understood, they will make us acquainted with certain precautions necessary in investigating the great question of specific inductive capacity.

1234. One of the inductive apparatus already described (1187, &c.) had a hemispherical cup of shell-lac introduced, which being in the interval between the inner ball and the lower hemisphere, nearly occupied the space there; consequently when the apparatus was charged, the lac was the dielectric or insulating medium through which the induction took place in that part. When this apparatus was first charged with electricity (1198.) up to a certain intensity, as 400°, measured by the COULOMB'S electrometer (1180.), it sank much faster from that degree than if it had been previously charged to a higher point, and had gradually fallen to 400°; or than it would do if the charge were, by a second application, raised up again to 400°; all other things remaining the same. Again, if after having been charged for some time, as fifteen or twenty minutes, it was suddenly and perfectly discharged, even the stem having all electricity removed from it (1203.), then the apparatus being left to itself, would gradually recover a charge, which in nine or ten minutes would rise up to 50° or 60°, and in one instance to 80°.

1235. The electricity, which in these cases returned from an apparently latent to a sensible state, was always of the same kind as that which had been given by the charge. The return took place at both the inducing surfaces; for if after the perfect discharge of the apparatus the whole was insulated, as the inner ball resumed a positive state the outer sphere acquired a negative condition.

1236. This effect was at once distinguished from that produced by the excited stem acting in curved lines of induction (1203. 1232.), by the circumstance that all the returned electricity could be perfectly and instantly discharged. It appeared to depend upon the shell-lac within, and to be, in some way, due to electricity evolved from it in consequence of a previous condition into which it had been brought by the charge of the metallic coatings or balls.

1237. To examine this state more accurately, the apparatus, with the hemispherical cup of shell-lac in it, was charged for about forty-five minutes to above 600° with positive electricity at the balls *h* and B (fig. 1.) above and within. It was then discharged, opened, the shell-lac taken out, and its state examined; this was done by bringing the carrier ball near the shell-lac, uninsulating it, insulating it, and then observing what charge it had acquired. As it would be a charge by induction, the state of the ball would indicate the opposite state of electricity in that surface of the shell-lac which had produced it. At first the lac appeared quite free from any charge; but gradually its two surfaces assumed opposite states of electricity, the concave surface, which had been next the inner and positive ball, assuming a positive state, and the convex surface, which had been in contact with the negative

coating, acquiring a negative state; these states gradually increasing in intensity for some time.

1238. As the return action was evidently greatest instantly after the discharge, I again put the apparatus together, and charged it for fifteen minutes as before, the inner ball positively. I then discharged it, instantly removing the upper hemisphere with the interior ball, and, leaving the shell-lac cup in the lower uninsulated hemisphere, examined its inner surface by the carrier ball as before (1237.). In this way I found the surface of the shell-lac actually *negative*, or in the reverse state to the ball which had been in it; this state quickly disappeared, and was succeeded by a positive condition, gradually increasing in intensity for some time, in the same manner as before. This first negative condition of the surface opposite the positive charging ball is a natural consequence of the state of things, the charging ball being in contact with the shell-lac only in a few points. It does not interfere with the general result and peculiar state now under consideration, except that it assists in illustrating in a very marked manner the ultimate assumption by the surfaces of the shell-lac of an electrified condition, similar to that of the metallic surfaces opposed to or against them.

1239. *Glass* was then examined with respect to its power of assuming this peculiar state. I had a thick flint glass hemispherical cup formed, which would fit easily into the space *o* of the lower hemisphere (1188. 1189.); it had been heated and varnished with a solution of shell-lac in alcohol, for the purpose of destroying the conducting power of the vitreous surface. Being then well warmed and experimented with, I found it could also assume the *same state*, but not apparently to the same degree, the return action amounting in different cases to quantities from 6° to 18°.

1240. *Spermaceti* experimented with in the same manner gave striking results. When the original charge had been sustained for fifteen or twenty minutes at about 500°, the return charge was equal to 95° or 100°, and was about fourteen minutes arriving at the maximum effect. A charge continued for not more than two or three seconds was here succeeded by a return charge of 50° or 60°. The observations formerly made (1234.) held good with this substance. Spermaceti, though it will insulate a low charge for some time, is a better conductor than shell-lac, glass, and sulphur; and this conducting power is connected with its readiness in exhibiting the particular effect under consideration.

1241. *Sulphur.*—I was anxious to obtain the amount of effect with this substance, first, because it is an excellent insulator, and in that respect would illustrate the relation of the effect to the degree of conducting power possessed by the dielectric (1247.); and in the next place, that I might obtain that body giving the smallest degree of the effect now under consideration, for the investigation of the question of specific inductive capacity (1277.).

1242. With a good hemispherical cup of sulphur cast solid and sound, I obtained the return charge, but only to an amount of 17° or 18°. Thus glass and sulphur,

which are bodily very bad conductors of electricity, and indeed almost perfect insulators, gave very little of this return charge.

1243. I tried the same experiment having *air* only in the inductive apparatus. After a continued high charge for some time I could obtain a little effect of return action, but it was ultimately traced to the shell-lac of the stem.

1244. I sought to produce something like this state with one electric power and without induction; for upon the theory of an electric fluid or fluids, that did not seem impossible, and then I should have obtained an absolute charge (1169. 1177.), or something equivalent to it. In this I could not succeed. I excited the outside of a cylinder of shell-lac very highly for some time, and then quickly discharging it (1203.), waited and watched whether any return charge would appear, but such was not the case. This is another fact in favour of the inseparability of the two electric forces, and another argument for the view that induction and its concomitant phenomena depend upon a polarity of the particles of matter.

1245. Although inclined at first to refer these effects to a peculiar masked condition of a certain portion of the forces, I think I have since correctly traced them to known principles of electrical action. The effects appear to be due to an actual penetration of the charge to some distance within the electric, at each of its two surfaces, by what we call conduction; so that, to use the ordinary phrase, the electric forces sustaining the induction are not upon the metallic surfaces only, but upon and within the dielectric also, extending to a greater or smaller depth from the metal linings. Let *c* (fig. 10.) be the section of a plate of any dielectric, *a* and *b* being the metallic coatings; let *b* be uninsulated, and *a* be charged positively; after ten or fifteen minutes, if *a* and *b* be discharged, insulated, and immediately examined, no electricity will appear in them; but in a short time, upon a second examination, they will appear charged in the same way, though not to the same degree, as they were at first. Now suppose that a portion of the positive force has, under the coercing influence of all the forces concerned, penetrated the dielectric and taken up its place at the line *p*, a corresponding portion of the negative force having also assumed its position at the line *n*; that in fact the electric at these two parts has become charged positive and negative; then it is clear that the induction of these two forces will be much greater one towards the other, and less in an external direction, now that they are at the small distance *n p* from each other, than when they were at the larger interval *a b*. Then let *a* and *b* be discharged; the discharge destroys or neutralizes all external induction, and the coatings are therefore found by the carrier ball unelectrified; but it also removes almost the whole of the forces by which the electric charge was driven into the dielectric, and though probably a part goes forward in its passage and terminates in what we call discharge, the greater portion returns on its course to the surfaces of *c*, and consequently to the conductors *a* and *b*, and constitutes the recharge observed.

1246. The following is the experiment on which I rest for the truth of this view.

Two plates of spermaceti, *d* and *f* (fig. 11.), were put together to form the dielectric, *a* and *b* being the metallic coatings of this compound plate, as before. The system was charged, then discharged, insulated, examined, and found to give no indications of electricity to the carrier ball. The plates *d* and *f* were then separated from each other, and instantly *a* with *d* was found in a positive state, and *b* with *f* in a negative state, nearly all the electricity being in the linings *a* and *b*. Hence it is clear that, of the forces sought for, the positive was in one half of the compound plate and the negative in the other half; for when removed bodily with the plates from each other's inductive influence, they appeared in separate places, and resumed of necessity their power of acting by induction on the electricity of surrounding bodies. Had the effect depended upon a peculiar relation of the contiguous particles of matter only, then each half plate, *d* and *f*, should have shown positive force on one surface and negative on the other.

1247. Thus it would appear that the best solid insulators, such as shell-lac, glass, and sulphur, have conductive properties to such an extent, that electricity can penetrate them bodily, though always subject to the overruling condition of induction (1178.). As to the depth to which the forces penetrate in this form of charge of the particles, theoretically, it should be throughout the mass, for what the charge of the metal does for the portion of dielectric next to it, should be done by the charged dielectric for the portion next beyond it again; but probably in the best insulators the sensible charge is to a very small depth only in the dielectric, for otherwise more would disappear in the first instance whilst the original charge is sustained, less time would be required for the assumption of the particular state, and more electricity would re-appear as return charge.

1248. The condition of *time* required for this penetration of the charge is important, both as respects the general relation of the cases to conduction, and also the removal of an objection that might otherwise properly be raised to certain results respecting specific inductive capacities, hereafter to be given (1269. 1277.).

1249. It is the assumption for a time of this charged state of the glass between the coatings in the Leyden jar, which gives origin to a well-known phenomenon, usually referred to the diffusion of electricity over the uncoated portion of the glass, namely, the *residual charge*. The extent of charge which can spontaneously be recovered by a large battery, after perfect uninsulation of both surfaces, is very considerable, and by far the largest portion of this is due to the return of electricity in the manner described. A plate of shell-lac six inches square, and half an inch thick, or a similar plate of spermaceti an inch thick, being coated on the sides with tin foil as a Leyden arrangement, will show this effect exceedingly well.

1250. The peculiar condition of dielectrics which has now been described, is evidently capable of producing an effect interfering with the results and conclusions drawn from the use of the two inductive apparatus, when shell-lac, glass, &c. is used in one or both of them (1192. 1207.): for upon dividing the charge in such cases ac-

cording to the method described (1198. 1207.), it is evident that the one just receiving its half charge must fall faster in its tension than the other. For suppose app. i. first charged, and app. ii. used to divide with it; though both may actually lose alike, yet app. i., which has been diminished one half, will be sustained by a certain degree of return action or charge (1234.), whilst app. ii. will sink the more rapidly from the coming on of the particular state. I have endeavoured to avoid this interference by performing the whole process of comparison as quickly as possible, and taking the force of app. ii. immediately after the division, before any sensible diminution of the tension arising from the assumption of the peculiar state could be produced; and I have assumed that as about three minutes pass between the first charge of app. i. and the division, and three minutes between the division and discharge, when the force of the non-transferable electricity is measured, the contrary tendencies for those periods would keep that apparatus in a moderately steady and uniform condition for the latter portion of time.

1251. The particular action described occurs in the shell-lac of the stems, as well as in the *dielectric* used within the apparatus. It therefore constitutes a cause by which the outside of the stems may in some operations become charged with electricity, independent of the action of dust or carrying particles (1203.).

¶ v. *On specific induction, or specific inductive capacity.*

1252. I now proceed to examine the great question of specific inductive capacity, i. e. whether different dielectric bodies actually do possess any influence over the degree of induction which takes place through them. If any such difference should exist, it appeared to me not only of high importance in the further comprehension of the laws and results of induction, but an additional and very powerful argument for the theory I have ventured to put forth, that the whole depends upon a molecular action, in contradistinction to one at sensible distances.

The question may be stated thus: suppose A an electrified plate of metal suspended in the air, and B and C two exactly similar plates, placed parallel to and on each side of A at equal distances and uninsulated; A will then induce equally towards B and C. If in this position of the plates some other dielectric than air, as shell-lac, be introduced between A and C, will the induction between them remain the same? Will the relation of C and B to A be unaltered, notwithstanding the difference of the dielectrics interposed between them?

1253. As far as I recollect, it is assumed that no change will occur under such variation of circumstances, and that the relations of B and C to A depend entirely upon their distance. I only remember one experimental illustration of the question, and that is by Coulomb*, in which he shows that a wire surrounded by shell-lac took exactly the same quantity of electricity from a charged body as the same wire in air. The experiment offered to me no proof of the truth of the supposition, for it is not

* Mémoires de l'Académie, 1787, pp. 452, 453.

the mere films of dielectric substances surrounding the charged body which have to be examined and compared, but the *whole mass* between that body and the surrounding conductors at which the induction terminates. Charge depends upon induction (1171. 1178.); and if induction relate to the particles of the surrounding dielectric, then it relates to *all* the particles of that dielectric inclosed by the surrounding conductors, and not merely to the few situated next to the charged body. Whether the difference I sought for existed or not, I soon found reason to doubt the conclusion that might be drawn from COULOMB'S result; and therefore had the apparatus made, which, with its use, has been already described (1187, &c.), and which appears to me well suited for the investigation of the question.

1254. Glass, and many bodies which might at first be considered as very fit to test the principle, proved exceedingly unfit for that purpose. Glass, principally in consequence of the alkali it contains, however well warmed and dried it may be, has a certain degree of conducting power upon its surface, dependent upon the moisture of the atmosphere, which renders it unfit for a test experiment. Resin, wax, naphtha, oil of turpentine, and many other substances were in turn rejected, because of a slight degree of conducting power possessed by them; and ultimately shell-lac and sulphur were chosen, after many experiments, as the dielectrics best fitted for the investigation. No difficulty can arise in perceiving how the possession of a feeble degree of conducting power tends to make a body produce effects, which would seem to indicate that it had a greater capability of allowing induction through it than another body perfect in its insulation. This source of error has been the one I have found most difficult to obviate in the proving experiments.

1255. *Induction through shell-lac.*—As a preparatory experiment, I first ascertained generally that when a part of the surface of a thick plate of shell-lac was excited or charged, there was no sensible difference in the character of the induction sustained by that charged part, whether exerted through the air in the one direction, or through the shell-lac of the plate in the other; provided the second surface of the plate had not, by contact with conductors, the action of dust, or any other means, become charged (1203.). Its solid condition enabled it to retain the excited particles in a permanent position, but that appeared to be all; for these particles acted just as freely through the shell-lac on one side as through the air on the other. The same general experiment was made by attaching a disc of tin foil to one side of the shell-lac plate, and electrifying it, and the results were the same. Scarcely any other solid substance than shell-lac and sulphur, and no liquid substance that I have tried, will bear this examination. Glass in its ordinary state utterly fails; yet it was essentially necessary to obtain this prior degree of perfection in the dielectric used, before any further progress could be made in the principal investigation.

1256. *Shell-lac and air* were compared in the first place. For this purpose a thick hemispherical cup of shell-lac was introduced into the lower hemisphere of one of the inductive apparatus (1187, &c.), so as nearly to fill the lower half of the space ρ, o

(fig. 1.) between it and the inner ball; and then charges were divided in the manner already described (1198. 1207.), each apparatus being used in turn to receive the first charge before its division by the other. As the apparatus were known to have equal inductive power when air was in both (1209. 1211.), any differences resulting from the introduction of the shell-lac would show a peculiar action in it, and if unequivocally referable to a specific inductive influence, would establish the point sought to be sustained. I have already referred to the precautions necessary in making the experiments (1199, &c.); and with respect to the error which might be introduced by the assumption of the peculiar state, it was guarded against, as far as possible, in the first place, by operating quickly (1248.), and afterwards by using that dielectric as glass or sulphur, which assumed the peculiar state most slowly, and in the least degree (1239. 1241.).

1257. The shell-lac hemisphere was put into app. i., and app. ii. left filled with air. The results of an experiment in which the charge through air was divided and reduced by the shell-lac app. were as follows:

App. i. Lac.		App. ii. Air.
Balls 255°.		
0°	. . . .	
	. . . .	304°
	. . . .	297
Charge divided.		
113	. . . .	
	. . . .	121
0	. . . .	after being discharged.
	. . . .	7 after being discharged.

1258. Here 297°, minus 7°, or 290°, may be taken as the divisible charge of app. ii. (the 7° being fixed stem action (1203. 1232.)), of which 145° is the half. The lac app. i. gave 113° as the power or tension it had acquired after division; and the air app. ii. gave 121°, minus 7°, or 114°, as the force it possessed from what it retained of the divisible charge of 290°. These two numbers should evidently be alike, and they are very nearly so, far indeed within the errors of experiment and observation. But these numbers differ very much from 145°, or the force which the half charge would have had if app. i. had contained air instead of shell-lac; and it appears that whilst in the division the induction through the air has lost 176° of force, that through the lac has only gained 113°.

1259. If this difference be assumed as depending entirely on the greater facility possessed by shell-lac of allowing or causing inductive action through its substance than that possessed by air, then this capacity for electric induction would be inversely as the respective loss and gain indicated above; and assuming the capacity of the air apparatus as 1, that of the shell-lac apparatus would be $\frac{176}{113}$, or 1·55.

1260. This extraordinary difference was so unexpected in its amount, as to excite the greatest suspicion of the general accuracy of the experiment, though the perfect discharge of app. i. after the division showed that the 113° had been taken and given up readily. It was evident that, if it really existed, it ought to produce corresponding effects in the reverse order; and that when induction through shell-lac was converted into induction through air, the force or tension of the whole ought to be *increased*. The app. i. was therefore charged in the first place, and its force divided with app. ii. The following were the results:

App. i. Lac.	App. ii. Air.
. . . .	0°
215°	
204	
Charge divided.	
. . . .	118
118	
. . . .	0 after being discharged.
0	after being discharged.

1261. Here 204° must be the utmost of the divisible charge. The app. i. and ii. present 118° as their respective forces; both now much *above* the half of the first force, or 102°, whereas in the former case they were below it. The lac app. i. has lost only 86°, yet it has given to the air app. ii. 118°, so that the lac still appears much to surpass the air, the capacity of the lac app. i. to the air app. ii. being as 1·37 to 1.

1262. The difference of 1·55 and 1·37 as the expression of the capacity for the induction of shell-lac seems considerable, but is in reality very admissible under the circumstances, for both are in error in *contrary directions*. Thus in the last experiment the charge fell from 215° to 204° by the joint effects of dissipation and absorption (1192. 1250.), during the time which elapsed in the electrometer operations, between the applications of the carrier ball required to give those two results. Nearly an equal time must have elapsed between the application of the carrier which gave the 204° result, and the division of the charge between the two apparatus; and as the fall in force progressively decreases in amount (1192.), if in this case it be taken at 6° only, it will reduce the whole transferable charge at the time of division to 198° instead of 204°; this diminishes the loss of the shell-lac charge to 80° instead of 86°; and then the expression of specific capacity for it is increased, and, instead of 1·37, is 1·47 times that of air.

1263. Applying the same correction to the former experiment in which air was *first* charged, the result is of the *contrary* kind. No shell-lac hemisphere was then in the apparatus, and therefore the loss would principally be from dissipation, and not from absorption: hence it would be nearer to the degree of loss shown by the numbers 304° and 297°, and being assumed as 6° would reduce the divisible charge to 284°. In

that case the air would have lost 170°, and communicated only 113° to the shell-lac; and the relative specific capacity of the latter would appear to be 1·50, which is very little indeed removed from 1·47, the expression given by the second experiment when corrected in the same way.

1264. The shell-lac was then removed from app. i. and put into app. ii. and the experiments of division again made. I give the results, because I think the importance of the point justifies and even requires them.

App. i. Air.	App. ii. Lac.
Balls 200°.	
. . . .	0°
286°	
283	
Charge divided.	
. . . .	110
109	
. . . .	0·25 after discharge.
Trace	after discharge.

Here app. i. retained 109°, having lost 174° in communicating 110° to app. ii.; and the capacity of the air app. is to the lac app., therefore, as 1 to 1·58. If the divided charge be corrected for an assumed loss of only 3°, being the amount of previous loss in the same time, it will make the capacity of the shell-lac app. 1·55 only.

1265. Then app. ii. was charged, and the charge divided thus:

App. i. Air.	App. ii. Lac.
0°	
. . . .	256°
. . . .	251
Charge divided.	
146	
. . . .	149
a little	after discharge.
. . . .	a little after discharge.

Here app. i. acquired a charge of 146°, while app. ii. lost only 102° in communicating that amount of force; the capacities being, therefore, to each other as 1 to 1·43. If the whole transferable charge be corrected for a loss of 4° previous to division, it gives the expression of 1·49 for the capacity of the shell-lac apparatus.

1266. These four expressions of 1·47, 1·50, 1·55, and 1·49 for the power of the shell-lac apparatus, through the different variations of the experiment, are very near to each other; the average is close upon 1·5, which may hereafter be used as the expression of the result. It is a very important result; and, showing for this parti-

cular piece of shell-lac a decided superiority over air in allowing or causing the act of induction, it proved the growing necessity of a more close and rigid examination of the whole question.

1267. The shell-lac was of the best quality, and had been carefully selected and cleaned; but as the action of any conducting particles in it would tend, virtually, to diminish the quantity or thickness of the dielectric used, and produce effects as if the two inducing surfaces of the conductors in that apparatus were nearer together than in the one with air only, I prepared another shell-lac hemisphere, of which the material had been dissolved in strong spirit of wine, the solution filtered, and then carefully evaporated. This is not an easy operation, for it is difficult to drive off the last portions of alcohol without injuring the lac by the heat applied; and unless they be dissipated, the substance left conducts too well to be used in these experiments. I prepared two hemispheres this way, one of them unexceptionable; and with it I repeated the former experiments with all precautions. The results were exactly of the same kind; the following expressions for the capacity of the shell-lac apparatus, whether it were app. i. or ii., being given directly by the experiments 1·46, 1·50, 1·52, 1·51; the average of these and several others being very nearly 1·5.

1268. As a final check upon the general conclusion, I then actually brought the surfaces of the air apparatus, corresponding to the place of the shell-lac in its apparatus, nearer together, by putting a metallic lining into the lower hemisphere of the one not containing the lac (1213.). The distance of the metal surface from the carrier ball was in this way diminished from 0·62 of an inch to 0·435 of an inch, whilst the interval occupied by the lac in the other apparatus remained 0·62 of an inch as before. Notwithstanding this change, the lac apparatus showed its former superiority; and whether it or the air apparatus was charged first, the capacity of the lac apparatus to the air apparatus was by the experimental results as 1·45 to 1.

1269. From all the experiments I have made, and their constant results, I cannot resist the conclusion that shell-lac does exhibit a case of *specific inductive capacity*. I have tried to check the trials in every way, and if not remove, at least estimate, every source of error. That the final result is not due to common conduction is shown by the capability of the apparatus to retain the communicated charge; that it is not due to the conductive power of inclosed small particles, by which they could acquire a polarized condition as conductors, is shown by the effects of the shell-lac purified by alcohol; and, that it is not due to any influence of the charged state, formerly described (1250.), first absorbing and then evolving electricity, is indicated by the *instantaneous* assumption and discharge of those portions of the power which are concerned in the phenomena, that effect occurring in these cases, as in all others of ordinary induction by charged conductors. The latter argument is the more striking in the case where the air apparatus is employed to divide the charge with the lac apparatus, for it obtains its portion of electricity in an *instant*, and yet is charged far above the *mean*.

1270. Admitting for the present the general fact sought to be proved; then 1·5, though it expresses the capacity of the apparatus containing the hemisphere of shell-lac, by no means expresses the relation of lac to air. The lac only occupies one half of the space *o*, *o*, of the apparatus containing it, through which the induction is sustained; the rest is filled with air, as in the other apparatus; and if the effect of the two upper halves of the globes be abstracted, then the comparison of the shell-lac powers in the lower half of the one, with the power of the air in the lower half of the other, will be as 2 : 1; and even this must be less than the truth, for the induction of the upper part of the apparatus, i. e. of the wire and ball B (fig. 1.) to external objects, must be the same in both, and considerably diminish the difference dependent upon, and really producible by, the influence of the shell-lac within.

1271. *Glass.*—I next worked with glass as the dielectric. It involved the possibility of conduction on its surface, but it excluded the idea of conducting particles within its substance (1267.) other than those of its own mass. Besides this it does not assume the charged state (1239.) so readily, or to such an extent as shell-lac.

1272. A thin hemispherical cup of glass being made hot was covered with a coat of shell-lac dissolved in alcohol, and after being dried for many hours in a hot place, was put into the apparatus and experimented with. It exhibited effects so slight, that, though they were in the direction indicating a superiority of glass over air, they were allowed to pass as possible errors of experiment; and the glass was considered as producing no sensible effect.

1273. I then procured a thick flint glass hemispherical cup resembling that of shell-lac (1239.), but not filling up the space *o*, *o*, so well. Its average thickness was 0·4 of an inch, there being an additional thickness of air, averaging 0·22 of an inch, to make up the whole space of 0·62 of an inch between the inducting metallic surfaces. It was covered with a film of shell-lac as the former was, (1272.) and being made very warm, was introduced into the apparatus, also warmed, and experiments made with it as in the former instances (1257. &c.). The general results were the same as with shell-lac, i. e. glass surpassed air in its power of favouring induction through it. The two best results as respected the state of the apparatus for retention of charge, &c., gave, when the air apparatus was charged first 1·336, and when the glass apparatus was charged first 1·45, as the specific inductive capacity for glass, both being without correction. The average of nine results, four with the glass apparatus first charged, and five with the air apparatus first charged, gave 1·38 as the power of the glass apparatus; 1·22 and 1·46 being the minimum and maximum numbers with all the errors of experiment upon them. In all the experiments the glass apparatus took up its inductive charge instantly, and lost it as readily; and during the short time of each experiment, acquired the peculiar state in a small degree only, so that the influence of this state, and also of conduction upon the results, must have been small.

1274. Allowing specific inductive capacity to be proved and active in this case, and

1·38 as the expression for the glass apparatus, then the specific inductive capacity of flint glass will be above 1·76, not forgetting that this expression is for a piece of glass of such thickness as to occupy not quite two-thirds of the space through which the induction is sustained (1273. 1253.).

1275. *Sulphur.*—The same hemisphere of this substance was used in app. ii. as was formerly referred to (1242.). The experiments were well made, i. e. the sulphur itself was free from charge both before and after each experiment, and no action from the stem appeared (1203. 1232.), so that no correction was required on that score. The following are the results when the air apparatus was first charged and divided:

App. i. Air.	App. ii. Sulphur.
Balls 280°.	
0°	
. . . . .	0°
438	
434	
Charge divided.	
. . . . .	162
164	
. . . . .	160
162	
. . . . .	0 after discharge.
0	after discharge.

Here app. i. retained 164°, having lost 270° in communicating 162° to app. ii., and the capacity of the air apparatus is to that of the sulphur apparatus as 1 to 1·66.

1276. Then the sulphur apparatus was charged first, thus:

. . . . .	0°
0°	
. . . . .	395
. . . . .	388
Charge divided.	
237	
. . . . .	238
0	after discharge.
. . . . .	0 after discharge.

Here app. ii. retained 238°, and gave up 150° in communicating a charge of 237° to app. i., and the capacity of the air apparatus is to that of the sulphur apparatus as 1 to 1·58. These results are very near to each other, and we may take the mean 1·62 as representing the specific inductive capacity of the sulphur apparatus; in which

case the specific inductive capacity of sulphur itself as compared to air = 1 (1270.) will be about or above 2·24.

1277. This result with sulphur I consider as one of the most unexceptionable. The substance when fused was perfectly clear, pellucid, and free from particles of dirt (1267.), so that no interference of small conducting particles confused the result. The body when solid is an excellent insulator, and by experiment was found to take up, with great slowness, that state (1241. 1242.) which alone seemed likely to disturb the conclusion. The experiments themselves, also, were free from any need of correction. Yet notwithstanding these circumstances, so favourable to the exclusion of error, the result is a higher specific inductive capacity for sulphur than for any other body as yet tried; and though this may in part be due to the sulphur being in a better shape, i. e. filling up more completely the space *o*, *o*, (fig. 1.) than the cups of shell-lac and glass, still I feel satisfied that the experiments altogether fully prove the existence of a difference between dielectrics as to their power of favouring an inductive action through them; which difference may, for the present, be expressed by the term *specific inductive capacity*.

1278. Having thus established the point in the most favourable cases that I could anticipate, I proceeded to examine other bodies amongst solids, liquids, and gases. These results I shall give with all convenient brevity.

1279. *Spermaceti.*—A good hemisphere of spermaceti being tried as to conducting power whilst its two surfaces were still in contact with the tin foil moulds used in forming it, was found to conduct sensibly even whilst warm. On removing it from the moulds and using it in one of the apparatus, it gave results indicating a specific inductive capacity between 1·3 and 1·6 for the apparatus containing it. But as the only mode of operation was to charge the air apparatus, and then after a quick contact with the spermaceti apparatus, ascertain what was left in the former (1281.), no great confidence can be placed in the results. They are not in opposition to the general conclusion, but cannot be brought forward as argument in favour of it.

1280. I endeavoured to find some liquids which would insulate well, and could be obtained in sufficient quantity for these experiments. Oil of turpentine, native naphtha rectified, and the condensed oil gas fluid, appeared by common experiments to promise best as to insulation. Being left in contact with fused carbonate of potassa, chloride of lime, and quick lime for some days and then filtered, they were found much injured in insulating power; but after distillation acquired their best state, though even then they proved to be conductors when large metallic contact was made with them.

1281. *Oil of turpentine rectified.*—I filled the lower half of app. i. with the fluid; and as it would not hold a charge sufficiently to enable me first to measure and then divide it, I charged app. ii. containing air, and dividing its charge with app. i. by a quick contact, measured that remaining in app. ii.: for, theoretically, if a quick contact would divide up to equal tension between the two apparatus, yet without sen-

sible loss from the conducting power of app. i.; and app. ii. were left charged to a degree of tension above half the original charge, it would indicate that oil of turpentine had less specific inductive capacity than air; or, if left charged below that mean state of tension, it would imply that the fluid had the greater inductive capacity. In an experiment of this kind, app. ii. gave as its charge 390° before division with app. i., and 175° afterwards, which is less than the half of 390°. Again, being at 175° before division, it was 79° after, which is also less than half the divided charge. Being at 79°, it was a third time divided, and then fell to 36°, less than the half of 79°. Such are the best results I could obtain; they are not inconsistent with the belief that oil of turpentine has a greater specific capacity than air, but they do not prove the fact, since the disappearance of more than half the charge may be due to the conducting power merely of the fluid.

1282. *Naphtha.*—This liquid gave results similar in their nature and direction to those with oil of turpentine.

1283. A most interesting class of substances, in relation to specific inductive capacity, now came under review, namely, the gases or aeriform bodies. These are so peculiarly constituted, and are bound together by so many striking physical and chemical relations, that I expected some remarkable results from them: air in various states was selected for the first experiments.

1284. *Air, rare and dense.*—Some experiments of division (1208.) seemed to show that dense and rare air were alike in the property under examination. A simple and better process was to attach one of the apparatus to an air pump, to charge it, and then examine the tension of the charge when the air within was more or less rarefied. Under these circumstances it was found, that commencing with a certain charge, that charge did not change in its tension or force as the air was rarefied, until the rarefaction was such that *discharge* across the space *o*, *o* (fig. 1.) occurred. This discharge was proportionate to the rarefaction; but having taken place, and lowered the tension to a certain degree, that degree was not at all affected by restoring the pressure and density of the air to their first quantities.

	inches of mercury.		
Thus at a pressure of . . .	30	the charge was . . .	88°
Again	30	the charge was . . .	88
Again	30	the charge was . . .	87
Reduced to	14	the charge was . . .	87
Raised again to	30	the charge was . . .	86
Being now reduced to . . .	3·4	the charge fell to . . .	81
Raised again to	30	the charge was still . .	81

1285. The charges were low in these experiments, first that they might not pass off at low pressure, and next that little loss by dissipation might occur. I now reduced them still lower, that I might rarefy further, and for this purpose in the following experiment used a measuring interval in the electrometer of only 15° (1185.). The pressure of air within the apparatus being reduced to 1·9 inches of mercury,

the charge was found to be 29°; then letting in air till the pressure was 30 inches, the charge was still 29°.

1286. These experiments were repeated with pure oxygen with the same consequences.

1287. This result of *no variation* in the electric tension being produced by variation in the density or pressure of the air, agrees perfectly with those obtained by Mr. Harris*, and described in his beautiful and important investigations contained in the Philosophical Transactions; namely that induction is the same in rare and dense air, and that the divergence of an electrometer under such variations of the air continues the same, provided no electricity pass away from it. The effect is one entirely independent of that power which dense air has of causing a higher charge to be retained upon the surface of conductors in it than can be retained by the same conductors in rare air; a point I propose considering hereafter.

1288. I then compared *hot and cold air* together, by raising the temperature of one of the inductive apparatus as high as it could be without injury, and then dividing charges between it and the other apparatus containing cold air. The temperatures were about 50° and 200°. Still the power or capacity appeared to be unchanged; and when I endeavoured to vary the experiment, by charging a cold apparatus and then warming it by a spirit lamp, I could obtain no proof that the inductive capacity underwent any alteration.

1289. I compared *damp and dry air* together, but could find no difference in the results.

1290. *Gases.*—A very long series of experiments was then undertaken for the purpose of comparing *different gases* one with another. They were all found to insulate well, except such as acted on the shell-lac of the supporting stem; these were chlorine, ammonia, and muriatic acid. They were all dried by appropriate means before being introduced into the apparatus. It would have been sufficient to have compared each with air; but, in consequence of the striking result which came out, namely, that *all had the same power of*, or *capacity for*, sustaining induction through them, (which perhaps might have been expected after it was found that no variation of density or pressure produced any effect,) I was induced to compare them, experimentally, two and two in various ways, that no difference might escape me, and that the sameness of result might stand in full opposition to the contrast of property, composition, and condition which the gases themselves presented.

1291. The experiments were made upon the following pairs of gases.

1. Nitrogen and . . . Oxygen.
2. Oxygen . . . . Air.
3. Hydrogen . . . Air.
4. Muriatic acid gas . Air.
5. Oxygen . . . . Hydrogen.
6. Oxygen . . . . Carbonic acid.

* Philosophical Transactions, 1834, pp. 223, 224, 237, 244.

7. Oxygen	Olefiant gas.
8. Oxygen	Nitrous gas.
9. Oxygen	Sulphurous acid.
10. Oxygen	Ammonia.
11. Hydrogen. . . .	Carbonic acid.
12. Hydrogen. . . .	Olefiant gas.
13. Hydrogen. . . .	Sulphurous acid.
14. Hydrogen. . . .	Fluo-silicic acid.
15. Hydrogen. . . .	Ammonia.
16. Hydrogen. . . .	Arseniuretted hydrogen.
17. Hydrogen. . . .	Sulphuretted hydrogen.
18. Nitrogen	Olefiant gas.
19. Nitrogen	Nitrous gas.
20. Nitrogen	Nitrous oxide.
21. Nitrogen	Ammonia.
22. Carbonic oxide . .	Carbonic acid.
23. Carbonic oxide . .	Olefiant gas.
24. Nitrous oxide . .	Nitrous gas.
25. Ammonia	Sulphurous acid.

1292. Notwithstanding the striking contrasts of all kinds which these gases present of property, of density, whether simple or compound, anions or cathions (665.), of high or low pressure (1284. 1286.), hot or cold (1288.), not the least difference in their capacity to favour or admit electrical induction through them could be perceived. Considering the point established, that in all these gases induction takes place by an action of contiguous particles, this is the more important, and adds one to the many striking relations which hold between bodies having the gaseous condition and form. Another equally important electrical relation, which will be examined in the next paper, is that which the different gases have to each other at the *same pressure* of causing the retention of the *same or different degrees of charge* upon conductors in them. These two results appear to bear importantly upon the subject of electro-chemical excitation and decomposition; for as *all* these phenomena, different as they seem to be, must depend upon the electrical forces of the particles of matter, the very distance at which they seem to stand from each other will do much, if properly considered, to illustrate the principle by which they are held in one common bond, and subject, as they must be, to one common law.

1293. It is just possible that the gases may differ from each other in their specific inductive capacity, and yet by quantities so small as not to be distinguished in the apparatus I have used. It must be remembered, however, that in the gaseous experiments the gases occupy all the space *o*, *o*, (fig. 1.) between the inner and the outer ball, except the small portion filled by the stem; and the results, therefore, are twice as delicate as those with solid dielectrics.

1294. The insulation was good in all the experiments recorded, except Nos. 10, 15, 21, and 25, being those in which ammonia was compared with other gases. When shell-lac is put into ammoniacal gas its surface gradually acquires conducting power, and in this way the lac part of the stem within was so altered, that the ammonia apparatus could not retain a charge with sufficient steadiness to allow of division. In these experiments, therefore, the other apparatus was charged; its charge measured and divided with the ammonia apparatus by a quick contact, and what remained untaken away by the division again measured (1281.). It was so nearly one half of the original charge, as to authorize, with this reservation, the insertion of ammoniacal gas amongst the other gases, as having equal power with them.

1295. Thus *induction* appears to be essentially an action of contiguous particles, through the intermediation of which the electric force, originating or appearing at a certain place, is propagated to or sustained at a distance, appearing there as a force of the same kind exactly equal in amount, but opposite in its direction and tendencies (1164.). Induction requires no sensible thickness in the conductors which may be used to limit its extent; an uninsulated leaf of gold may be made very highly positive on one surface, and as highly negative on the other, without the least interference of the two states whilst the inductions continue. Nor is it affected by the nature of the limiting conductors, provided time be allowed, in the case of those which conduct slowly, for them to assume their final state (1170.).

1296. But with regard to the *dielectrics* or insulating media, matters are very different (1167.). Their thickness has an immediate and important influence on the degree of induction. As to their quality, though all gases and vapours are alike, whatever their state, amongst solid bodies, and between them and gases, there are differences which prove the existence of *specific inductive capacities*, these differences being in some cases very great.

1297. The direct inductive force, which may be conceived to be exerted in lines between the two limiting and charged conducting surfaces, is accompanied by a lateral or transverse force equivalent to a dilatation or repulsion of these representative lines (1224.); or the attractive force which exists amongst the particles of the dielectric in the direction of the induction is accompanied by a repulsive or a diverging force in the transverse direction (1304.).

1298. Induction appears to consist in a certain polarized state of the particles, into which they are thrown by the electrified body sustaining the action, the particles assuming positive and negative points or parts, which are symmetrically arranged with respect to each other and the inducting surfaces or particles*. The state must be a

* The theory of induction which I am stating does not pretend to decide whether electricity be a fluid or fluids, or a mere power or condition of recognised matter. That is a question which I may be induced to consider in the next or following series of these researches.

forced one, for it is originated and sustained only by force, and sinks to the normal or quiescent state when that force is removed. It can be *continued* only in insulators by the same portion of electricity, because they only can retain this state of the particles (1304.).

1299. The principle of induction is of the utmost generality in electric action. It constitutes charge in every ordinary case, and probably in every case; it appears to be the cause of all excitement, and to precede every current. The degree to which the particles are affected in this their forced state, before discharge of one kind or another supervenes, appears to constitute what we call *intensity*.

1300. When a Leyden jar is *charged*, the particles of the glass are forced into this polarized and constrained condition by the electricity of the charging apparatus. *Discharge* is the return of these particles to their natural state from their state of tension, whenever the two electric forces are allowed to be disposed of in some other direction.

1301. All charge of conductors is on their surface, because being essentially inductive, it is there only that the medium capable of sustaining the necessary inductive state begins. If the conductors are hollow and contain air or any other dielectric, still no *charge* can appear upon that internal surface, because the dielectric there cannot assume the polarized state throughout, in consequence of the opposing actions in different directions.

1302. The known influence of *form* is perfectly consistent with the corpuscular view of induction set forth. An electrified cylinder is more affected by the influence of the surrounding conductors (which complete the condition of charge) at the ends than at the middle, because the ends are exposed to a greater sum of inductive forces than the middle; and a point is brought to a higher condition than a ball, because, by relation to the conductors around, more inductive force terminates on its surface than on an equal surface of the ball with which it is compared. Here too, especially, can be perceived the influence of the lateral or transverse force (1297.), which, being a power of the nature of or equivalent to repulsion, causes such a disposition of the lines of inductive force in their course across the dielectric, that they must accumulate upon the point, the end of the cylinder, or any projecting part.

1303. The influence of *distance* is also in harmony with the same view. There is perhaps no distance so great that induction cannot take place through it*; but with the same constraining force (1298.) it takes place the more easily, according as the extent of dielectric through which it is exerted is lessened. And as it is assumed by the theory that the particles of the dielectric, though tending to remain in a normal state, are thrown into a forced condition during the induction; so it would seem to

* I have traced it experimentally from a ball placed in the middle of the large cube formerly described (1173.) to the sides of the cube six feet distant, and also from the same ball placed in the middle of our large lecture-room to the walls of the room at twenty-six feet distance, the charge upon the ball in these cases being solely due to induction through these distances.

follow that the fewer there are of these intervening particles opposing their tendency to the assumption of the new state, the greater degree of change will they suffer, i. e. the higher will be the condition they assume, and the larger the amount of inductive action exerted through them.

1304. I have used the phrases *lines of inductive force* and *curved lines* of force (1231. 1297. 1298. 1302.) in a general sense only, just as we speak of the lines of magnetic force. The lines are imaginary, and the force in any part of them is of course the resultant of compound forces, every molecule being related to every other molecule in *all* directions by the tension and reaction of those which are contiguous. The transverse force is merely this relation considered in a direction oblique to the lines of inductive force, and at present I mean no more than that by the phrase. With respect to the term *polarity* also, I mean at present only a disposition of force by which the same molecule acquires opposite powers on different parts. The particular way in which this disposition is made will come into consideration hereafter, and probably varies in different bodies, and so produces variety of electrical relation. All I am anxious about at present is, that a more particular meaning should not be attached to the expressions used than I contemplate. Further inquiry, I trust, will enable us by degrees to restrict the sense more and more, and so render the explanation of electrical phenomena day by day more and more definite.

1305. As a test of the probable accuracy of my views, I have throughout this experimental examination compared them with the conclusions drawn by M. Poisson from his beautiful mathematical inquiries*. I am quite unfit to form a judgment of these admirable papers; but as far as I can perceive, the theory I have set forth and the results I have obtained are not in opposition to such of those conclusions as represent the final disposition and state of the forces in the limited number of cases he has considered. His theory assumes a very different mode of action in induction to that which I have ventured to support, and would probably find its mathematical test in the endeavour to apply it to cases of induction in curved lines. To my feeling it is insufficient in its mode of accounting for the retention of electricity upon the surface of conductors by the pressure of the air, an effect which I hope to show is simple and consistent according to the present view; and it does not touch voltaic electricity, or in any way associate it and what is called ordinary electricity under one common principle.

I have also looked with some anxiety to the results which that indefatigable philosopher Harris has obtained in his investigation of the laws of induction†, knowing that they were experimental, and having a full conviction of their exactness; but I am happy in perceiving no collision at present between them and the views I have set forth.

1306. Finally, I beg to say that I put forth my particular view with doubt and fear,

* Mémoires de l'Institut, 1811, tom. xii. the first page 1, and the second paging 163.

† Philosophical Transactions, 1834, p. 213.

lest it should not bear the test of general examination, for unless true it will only embarrass the progress of electrical science. It has long been on my mind, but I hesitated to publish it until the increasing persuasion of its accordance with all known facts, and the manner in which it linked together effects apparently very different in kind, urged me to write the present paper. I as yet see no inconsistency between it and nature, but, on the contrary, think I perceive much new light thrown by it on her operations; and my next papers will be devoted to a review of the phenomena of conduction, electrolyzation, current, magnetism, retention, discharge, and some other points, with an application of the theory to these effects, and an examination of it by them.

Royal Institution,
November 16, 1837.

II. *Fourth Letter on Voltaic Combinations, with reference to the Mutual Relations of the Generating and Conducting Surfaces. Addressed to* Michael Faraday, *Esq. D.C.L. F.R.S., Fullerian Prof. Chem. Royal Institution, &c. &c. &c. By* J. Frederic Daniell, *F.R.S., Prof. Chem. in King's College, London.*

Received January 18,—Read January 25, 1838.

My dear Faraday,

In my second* letter to you upon Voltaic Combinations, I suggested that, in a theoretical point of view, the most simple and perfect combination would probably consist of a solid sphere (or rather active point) of a generating metal, surrounded by a hollow sphere of an inactive conducting metal, with an intervening liquid electrolyte; the circuit being completed by a conducting wire properly disposed for connecting the two metals. Further reflection led me to believe that a series of experiments commencing as nearly as possible with these most simple conditions of the problem, might throw some light upon the relative dimensions, positions and actions of the generating and conducting plates of voltaic combinations in general; which appeared to me not to have received all the elucidation of which they might be susceptible. The subject, it is true, has not escaped the attention of experimenters; but most of the results with which I am acquainted are so involved in the errors arising from the variable condition of the source of the current itself, which I have already pointed out, as to leave it in a very unsatisfactory state. I moreover proposed to myself, in the investigation which I consequently undertook, to trace the self-distribution of the force from its origin, by the indications of reduced copper, in the manner described in my first letter†. I have thus been led to some results which I trust you will consider of sufficient importance to justify my troubling you with another communication upon the subject.

My first apparatus, which it is necessary to describe, consists of two hollow hemispheres of brass, fitting together water-tight by means of exterior flanges half an inch wide, and a collar of leather, and thus forming a sphere, the interior diameter of which is $9\frac{1}{4}$ inches; consequently exposing a surface of about 268·8 square inches. The lower hemisphere is fitted into a frame carrying buttons, by which the upper can be securely wedged down upon it; and underneath there is a small cock, by which any liquid in the interior may be drawn off. The upper part of the upper hemisphere terminates in a tube of about one inch in length, forming an opening through which a membranous bag may be introduced, and from which it may be suspended. The

* Philosophical Transactions, 1836, p. 128. † Ibid. p. 113.

electrolyte which I employed in all the following experiments was the same as that which I employ in the constant battery, viz. eight parts of water, by measure, to one part of oil of vitriol in contact with the generating surface, and the same diluted acid, further saturated with sulphate of copper, in contact with the conducting surface.

The measures of the force to which I had recourse were, in the first instance, a coarse galvanometer consisting of a single needle seven inches in length, with a coil of twenty turns of wire $\frac{1}{10}$th of an inch in diameter. The large scale of the experiments precluded me from employing delicate instruments with astatic needles. Being dissatisfied with this measure, I afterwards employed a Bréguet's thermometer fitted up according to the plan of Professor De la Rive*, which measures the force of a current which passes through it by the differences of its heating power upon the compound spiral of platinum and silver of which it consists, the degrees into which it is divided being directly proportional to such differences. I had every reason to be satisfied with this instrument; and have no doubt of its accuracy, in the case, at least, of currents of such low tension as those for which alone I have hitherto employed it.

In my first experiment the sphere was charged with the solution of copper, and the membrane with the plain acid; a small sphere of amalgamated zinc, one inch diameter (and exposing a surface therefore of 3·14 square inches), was suspended by means of a well-varnished copper wire in the centre of the latter: the other extremity of the wire was connected with one of the cups of the galvanometer, and the circuit was completed by a wire leading from the other cup to a small mercury cup upon the upper brass hemisphere, placed at a distance of two inches from the tube. The deviation of the needle was 60°; and it remained steady for a long period, during which the experiment was repeated and varied. When, instead of the galvanometer, the circuit was closed with a piece of platinum wire one inch in length and $\frac{1}{100}$ inch diameter, it continued red hot for a period of five hours. The circuit remained closed for seventeen hours, and the apparatus was then opened and examined. The zinc ball had dropped off the wire, and was reduced to about one half of its original size. The upper hemisphere was found coated with reduced copper, beautifully marked half-way up from the equator with concentric circles of alternate dark and light stripes of pink and red; these were followed by a broad even band of pink, which reached to a circle within $1\frac{1}{2}$ inch of the aperture, which was composed of the unchanged surface of the brass, and which evidently had not been in contact with the liquid. The lower hemisphere, which had been insulated from the upper by the collar of leather, had no copper precipitated upon it.

The sphere was again put together, and charged as before, with a new zinc ball. The circuit was closed, as in the first experiment, with the galvanometer in contact with the upper hemisphere, and the deviation of the needle was 60°.

The connexion was then broken with the upper hemisphere, and made with the bottom of the lower hemisphere; the deviation was again 60°.

* Mémoires de la Soc. de Phys. de Génève, 1836, p. 140.

When connexion was made at the same time with both hemispheres, the deviation was the same. If, while in this state, either wire were lifted singly from its connexion, the needle remained perfectly steady.

When the wire from the zinc ball was lifted from the cup, there was a spark upon breaking the circuit; and there was also a spark when the wires from the two hemispheres were disconnected from the circuit together, but not when lifted singly.

Upon leading the conducting wire of the lower hemisphere into direct communication with that of the zinc ball, while the wire of the upper hemisphere was connected as before with the galvanometer, or *vice versâ*, the needle only receded to 40°. From this it appeared that, although the whole amount of force originating at the zinc was capable of passing off by means of either hemisphere singly, when both passages were open it distributed itself between the two, notwithstanding the additional resistance in one by the interposition of the galvanometer.

Extra contacts made with different parts of the two hemispheres made no difference in any of the preceding experiments.

The apparatus was now left for eighteen hours with both hemispheres in connexion with one extremity of the galvanometer, and the zinc ball with the other. Upon examination the needle was found still deflected 25°; and upon agitation of the zinc ball it rose to 55°, but almost immediately declined to 25°. The acid had become nearly saturated with oxide of zinc. The sphere was opened, and both hemispheres exhibited a perfectly even coating of beautiful pink copper, through which the rings of the upper hemisphere were however still discernible.

It will perhaps be advantageous, before I proceed further, to collect into one point of view the principal conclusions which, I conceive, may be drawn from the preceding experiments with the spherical combination.

1st. The force emanating from the active zinc centre diffused itself over every part of the upper hemisphere, from which there was a good conducting passage for its circulation, to an amount which is measured by 60° of the galvanometer; and which was capable of maintaining an inch of platinum wire $\frac{1}{130}$ inch diameter at red heat.

2nd. The same amount of force was maintained by either hemisphere indifferently; but when both conducting hemispheres were in metallic communication there was no increase of force. The transfer of half the power from one hemisphere to the other occupied no appreciable interval of time.

3rd. Although the force was not increased it spread itself equally over the whole sphere, as manifested by the diffusion of the precipitated copper.

4th. When one hemisphere was connected with the zinc centre by a short wire capable of affording circulation to the whole force, and the other hemisphere was connected by a long wire through the galvanometer with the same centre, the diffusion of the force over the whole sphere was maintained, although the half of it was obliged to overcome the much greater resistance of the longer circuit.

5th. There was no greater accumulation of precipitated copper about the points

with which the conducting wires were brought into contact, and towards which the force diffused over the sphere must have converged, than at any other point; proving that the force must have diverged from the centre equally through the electrolyte, and could only have drawn towards the conducting wires in the conducting sphere itself.

I now destroyed the insulation of the two hemispheres by fixing to the lower one a thin ring of brass, which came in contact with the upper when wedged down in its position by the means already described. The sphere, thus in good metallic communication in every part, was charged as before; the precipitated copper having been previously cleaned off by a little nitric acid and rotten-stone. The deviation of the galvanometer was 55°, and it was perfectly steady, whatever number of connexions were made with the sphere, or at whatever point the circuit was completed. It made no difference in the amount of the force which circulated, whether the contact with the brass was made as near as possible to the zinc conducting wire, or at the point the farthest removed from it.

I next proceeded to ascertain what would be the effect of increasing the surface of the generating metal. For this purpose two amalgamated zinc balls of the same diameter as before were placed in contact upon a varnished wire, and substituted for the single ball in the centre of the sphere: the deviation of the galvanometer only increased 5°, rising from 55° to 60°. Upon replacing the single ball it fell again to 55°, and so alternately rose and fell to the same amount upon frequent repetitions of the change. An amalgamated zinc rod six inches in length, and $\frac{1}{3}$ inch in diameter, was then substituted for the balls, but the deviation did not rise higher than 60°. Thus the generating surface was increased from about three square inches to six and 9·4 with very little increased effect.

These experiments were all made with the generating metal placed as nearly as possible in the centre of the conducting sphere: this position was now changed, and sometimes the zinc ball was placed at the bottom of the membrane almost in contact with the sphere; sometimes it was drawn up nearly to the top; again it was placed in the centre; but none of these changes produced any appreciable alteration in the deviation of the needle, the galvanometer constantly indicating 55°.

From this I was led to consider (although I am quite aware that the measures may not have been sufficiently accurate to determine the point with precision,) whether the force emanating from the zinc ball might not diffuse itself over the surrounding conducting sphere in obedience to the well-known law of radiant forces of the inverse square of the distance; since, although the cases may be dissimilar, according to this law an attractive point placed within a hollow sphere of attractive matter remains in equilibrio, whatever its position may be. Or the analogy, perhaps, is stronger of light diffusing itself from a luminous point within the sphere in the same relative positions as those of the generating ball.

This conclusion, if correct, appearing to me to be of great importance, I was desirous of confirming it by repetition and variations of the experiment.

For this purpose I made use of the calorific galvanometer (as it has been named by M. De la Rive,) instead of the magnetic; and the zinc ball was a little larger than in the preceding experiments, measuring $1\frac{1}{2}$ inch in diameter, and presenting therefore a surface of 3·94 square inches. The sphere was charged as before, and when the ball was placed in the centre

The index advanced	90°
When drawn nearly to the top	90
When thrust nearly to the bottom . . .	87

The index remained perfectly steady during several repetitions of the experiments; and when the connexion with the brass sphere was changed to opposite points. There can be no doubt that these results indicate the same equality of action as before; the slight difference in the lower position being, probably, owing to accidental circumstances, such as the unequal thickness of the membrane at that point, or the speedy saturation of the acid when the solution cannot fall away from the zinc. Thus it would appear, that in none of these positions was there any virtual approximation of the generating and conducting surfaces. The diffusion of the precipitated copper was, however, very much influenced by the position of the ball; when near the top or the bottom, it was thrown down in a compact layer on a segment immediately in its vicinity, and became thinner and thinner over the more remote parts of the sphere. It was only when exactly in the centre that the diffusion was perfectly equal. To observe this effect with the greatest distinctness, the action in each case should not be allowed to continue more than ten or fifteen minutes; for the deposition becomes compact in every part in a longer time, when it is not so easy to judge of the difference of thickness.

My next step was to measure the effects of different portions of the sphere in combination with the zinc ball. I took the lower hemisphere alone of the apparatus and filled it with the solution of copper, and placing a wooden bar across it which supported the membranous bag to carry the acid, I had it in my power to immerse the ball in any required position.

I first placed the same ball as in the last experiment just below the surface exactly in the centre, and the calorific galvanometer indicated 90°: upon removing it within half an inch of the bottom it rose to 115°.

On another occasion I placed the ball in three different positions upon the surface of the liquid in the hemisphere, namely, in the centre and close to each side: in each position the instrument marked 86°. Upon lowering it as close as possible to the bottom it rose to 100°. In all these experiments the precipitated copper was diffused over the whole hemisphere.

It is worthy of remark, that the amount of force thus called into action at the surface of the liquid in the hemisphere, is nearly the same as that from the whole charged sphere, or either of its hemispheres.

These results, I conceive, are not in opposition to the law of radiant forces sug-

gested by the experiments with the entire sphere; although I cannot vouch for the comparative measures being absolutely correct or uninfluenced, in some degree, from day to day by extraneous circumstances, and particularly by changes of temperature: nor can it, indeed, be expected that the experimental deductions from a ball of the dimensions which I employed can do more than approximate to the mathematical demonstrations of the relations of an active point within an attractive sphere.

An accidental circumstance next furnished me with an interesting variation in the combination of the whole sphere. My intention had been to fill it as before, and repeat the experiments; but I afterwards found that the liquid only reached to within 45° from the vertex. The zinc ball which I made use of was $\frac{1}{4}$ inch in diameter, and the membrane was full of the dilute acid. The experiments were made in three different positions of the ball, and with three variations of the wire connecting the circuit with the brass sphere.

The first connexion was made near the top at a point which was not within the contact of the included liquid; the results were

	Caloric Galv.
At the top	45°
Centre	55
Bottom	65

Connexion with the bottom of sphere.

Ball at the top	45°
Ball at the centre	70
Ball at the bottom	77

Connexion with the sphere both at the top and bottom.

Ball at the top	45°
Ball at the centre	73
Ball at the bottom	78

The two last series may be taken to be identical, but I am at a loss to explain the difference of the first series from these two. It is probable that the zinc ball, though wholly immersed in the acid in the membrane, was not below the surface of the exterior solution in the sphere. The results confirm in a general way the conclusions which have been previously suggested.

There was yet another combination which I thought it desirable to try; namely, with the generating ball placed within the charged sphere with its two hemispheres insulated. The zinc ball which I employed for this experiment was one inch diameter, and I tried it first with the standard acid with which all the other experiments had been made, and repeated it with the same acid diluted with an equal bulk of water, in order that I might ascertain whether the law of action were influenced at all by the amount of force put into circulation. The results were perfectly consistent with each other.

	Standard Acid.	Dilute Acid.
	°	°
The zinc ball placed near the bottom, connexion with top insulated hemisphere	52	42
Connected with bottom	59	50
The zinc ball placed near the top, connexion with bottom insulated hemisphere	51	41
Connected with top	59	50
Both hemispheres connected	59	

In these experiments, that hemisphere alone with which the connexion had been made could have influenced the results as a conducting surface.

A question now occurred to me of extreme interest: admitting that the force generated in the preceding combinations where it was limited in its diffusion by the concave surfaces of spherical forms, follows in its action the law of radiant forces, we know that it is not a *simple* radiant force like that of gravity, but a molecular force propagated from particle to particle, and possibly modified by other forces with which the same particles may be endowed. The law of radiation, if established, may, therefore, be the result of the concurrence of more than one force, limited by the spherical combinations with which we have been dealing. The physical law, in ordinary cases, supposes both the sphere and the point within to consist of similar attractive matter; and were the force which we are now investigating of the same nature, it would make no difference whether the sphere consisted of merely conducting metal and a small interior generating ball, or whether the sphere consisted of generating metal and the ball were an inactive conductor,—inactive I mean with regard to the electrolyte. What experimental results, therefore, would the latter combination afford?

I caused two hemispheres of zinc to be made of exactly the same form and dimensions as those of copper, and fitted together in the same way; the interior surfaces of these were thoroughly amalgamated with mercury. When wedged together by their flanges and ring of leather, metallic contact was preserved between the two by a small interior ring of amalgamated zinc. The sphere was charged with dilute acid, and the interior membranous bag with the acid solution of copper. A copper ball of one inch diameter fixed to the end of a varnished wire was then immersed in the latter, and a circuit formed by contact with different parts of the zinc sphere with the intervention of the calorific galvanometer.

The indications of the instrument were as follow:—

Ball at the top	36°
Ball at the centre	36
Ball at the bottom	36
Again at the top	35

The index was steady for a short time, but began slowly to decline in each instance for about 10°. Upon agitation of the ball in the solution it always rose to its previous amount.

Upon repeating the experiment and keeping the ball always agitated, the following results were obtained:—

Ball at the top	40°
Ball at the centre	42
Ball at the bottom	42

Another repetition gave

Ball at the top	45
Ball at the centre	46
Ball at the bottom	44

The index always fell when the ball was not agitated, and the decline may probably be ascribed to a change in the saturation of the liquid in immediate contact with the ball, which agitation prevented by keeping the solution in an uniform state.

I now separated the hemisphere and experimented with the lower one alone. The results were as follow:—

Ball at the top	35°
Ball at the bottom	80
Ball at the top	35
Ball half-way between	60

When the ball was placed at the centre of the surface and at the sides almost in contact with the hemisphere, the index rose to 36° in all the three positions.

Hence it appears that the same law was maintained; although the force which circulated was reduced to one half of the amount of that from the first combination.

Before I venture to offer two or three remarks upon this difference, I shall proceed to lay before you the results of some experiments upon combinations of generating and conducting surfaces of other forms, commencing with the cylindrical as approaching in simplicity to the spherical, and for the purpose of connecting my observations with that form which I have found most practically advantageous in the construction of my battery.

I took one of the cells of the small battery*, six inches in height and eleven inches in circumference, charged in the usual way, and found that the single zinc ball of one inch diameter, produced with it a deflection of the magnetic galvanometer of 55°. Two similar balls only increased the deflection to 60°, and its own rod, six inches in length and $\frac{1}{8}$ inch diameter, did not increase the effect. These were exactly the same amounts as were produced by the same generating surfaces in the brass sphere.

An amalgamated zinc cell of exactly the same dimensions, charged with the di-

* Philosophical Transactions, 1836, p. 117.

luted acid and solution of copper in the membrane, with a copper ball one inch diameter, deflected the galvanic needle 45°.

With the calorific galvanometer it moved the index to 25°.

Being desirous of ascertaining whether the same difference between the reversed combinations would exist in a series as in single circuits, I compared together three copper cylinders with three zinc rods, and three zinc cylinders with copper rods, by means of a voltameter. The former produced in $\frac{1}{4}$ hour 3·6 cubic inches of gas. The latter produced in $\frac{1}{4}$ hour 1·75 cubic inches: indicating, as before, that when the generating surface constituted the circumference of the arrangement, the force was only the half of that which was evolved when it formed the centre.

In the copper cylinder it made no difference whether the zinc ball or rod were placed in the centre, or nearly in contact with the side.

I next took an oval copper plate, the diameters of which were $13\frac{1}{2}$ inches by 10 inches, and soldered a copper wire at one extremity of the longest diameter, in a perpendicular position, and placing it in an earthen pan, covered it with a depth of $4\frac{1}{2}$ inches of the acid solution of copper. Over its centre I suspended, by means of a cross bar, a bag of membrane filled with the dilute acid; and nearly at the bottom of this I placed an amalgamated zinc ball, connected with a varnished copper wire. I then formed a circuit by means of the magnetic galvanometer, and the needle was deflected 55°. It remained perfectly steady for half an hour, when the plate was taken out and examined. It was found covered with fresh precipitated copper, the coat being a little thicker at the centre, and becoming thinner by almost insensible gradations towards the edge. At one point it had begun to turn round the edge and to diffuse itself on the under side.

The zinc ball was next placed just below the surface of the liquid in the pan, so that its least distance from the conducting plate was the same as when placed in the centre of the sphere. Upon completing the circuit the needle was deflected 45°.

The zinc ball was now drawn up in the tube so as to remain immersed in the acid, but two inches above the level of the solution; the needle was still deflected 35°. Upon again replacing it below the level it returned to 45°, and fell to the same amount upon once more drawing it up. In this case the lateral diffusion of the efficient force must have been prevented for the first two inches of its course; the mode of its after propagation was the subject of my next inquiry. While the apparatus was in this position, one zinc rod, and afterwards two, connected together, were immersed into the acid, so as to extend through the whole depth of the solution, without deflecting the needle more than 55°.

In these experiments the stem of the copper wire which was soldered to the copper plate was covered with precipitated copper as well as the plate. In subsequent experiments it was protected by varnish.

In order effectually to cut off the lateral radiation from the zinc ball, I placed it in a glass tube, six inches long; and over the lower aperture, which was $1\frac{1}{2}$ inch

diameter, I tied a piece of membrane: the tube was then filled with dilute acid and plunged into the solution of copper contained in the brass hemisphere, against the bottom of which it rested. The ball was supported within half an inch of the diaphragm. Upon closing the circuit by means of the magnetic galvanometer, the needle indicated 40°. At the expiration of five minutes the solution was drawn off and the hemisphere examined: there was found a beautiful well-defined circle of pink copper, two inches diameter, surrounded by a halo of darker colour, evidently of fresh precipitated copper of less thickness, but not extending over more than a fourth part of the surface. From this experiment it was evident that the force had diverged from the aperture of the glass tube, as from a centre, after it had entered the solution; the circle of pink copper being of a diameter half an inch greater than the aperture, and the fainter halo extending some inches around.

I now moved the tube into such a position, that, the zinc ball, remaining where it was in the tube, might be just below the level of the solution in the hemisphere; and now, notwithstanding the greater distance at which it was placed from the conducting surface, the needle rose to 45°, and the precipitated copper made its appearance over the whole hemisphere.

While the zinc ball was in its last position, the glass tube itself was pressed down till it again rested upon the bottom of the hemisphere; all lateral diffusion was thus cut off for a distance of $4\frac{1}{2}$ inches, through which the force was propagated, and it could only spread after it emerged from the tube; the galvanometer indicated 30°. When the glass screen, as it may be called, was again drawn up, the needle returned to 45°.

Similar experiments were often repeated with the substitution of large flat plates for the hemisphere. When the zinc ball was thus confined in a tube with a diaphragm, and placed within an inch of the plate, the precipitation always commenced with a circle a little larger than the aperture, and gradually extended itself, so that after some hours action it formed a circle of four or five inches diameter, and sometimes turned the edge, and made its appearance on the under surface.

Being desirous of ascertaining to what amount the under surface of a plate thus immersed in the electrolyte would affect the action, I covered the upper surface of the oval copper plate with lac varnish, and replaced it in its pan; I then placed the zinc ball in the tube within half an inch of the diaphragm, and plunged it just below the surface of the solution; upon completing the circuit the needle indicated 50°. In ten minutes time I examined the plate, and found the under side covered with beautiful pink copper, with the exception of an irregular oval space, whose diameters were about $4\frac{1}{2}$ inches by 4. The precipitation had evidently begun upon the edge, where it was thickest.

The plate was returned to its position, and the galvanometer again indicated 50°. When a double ball was substituted for the single it rose to 55°. It was again examined after an hour's action, and on the under side presented the appearance of a

border of compact pink copper, varying from $1\frac{1}{2}$ inch to $\frac{7}{8}$ inch in width, but not wider at the sides than the ends; and the remainder was covered with precipitated copper of a darker red colour, into which the former graduated. On the upper varnished side there were a number of little rosettes of copper, which were loose, giving it a gritty feel, and which evidently proceeded from a number of minute points to which the varnish had not adhered. Fig. 1. (Plate II.) may convey an idea of the appearance of the precipitated copper. Both surfaces of the copper plate having been cleaned, it was replaced in the solution, and the double zinc ball in the membrane, instead of being placed over the centre, was suspended over that end of the oval which was farthest removed from the conducting wire. The galvanometer indicated 48°, and remained perfectly steady for an hour, during which the action was continued. At the expiration of this time the plate was examined, and the pink copper was found deposited only at the extremity under the ball, forming a segment of a circle, of which the ball was the centre, but still not sharply defined, but imperceptibly shading off by a darker colour to the unchanged surface at the other extremity.

Here the efficiency of the generating ball was evidently impeded by the deficiency of the conducting plate on one side. The radiation could only take place towards the other side; and although the conductor was of great extent, the increasing distance of its several points caused it to act at a disadvantage.

After these experiments the plate was cleaned and replaced in the solution; and in order to determine the action of more than one generating point, at a distance from one another upon the same conducting plate, I first placed a single zinc ball in a membrane close to the conducting wire of the plate, and found the deviation of the galvanometer 42°·5. I then broke this connexion, and placed a double ball of zinc over the farthest extremity of the oval; the galvanometer again marked 42°·5. When the circuit was completed with both together the galvanometer only rose to 45°. Upon making or breaking contact with either ball singly, while the other remained in circuit, the needle only varied 2° or 3°. When the positions of the double and single ball were changed, the difference upon breaking the contact with it was 5°. After four hours action the plate was examined, and the pink copper was found deposited in two segments of circles, of which the balls had been the respective centres; and the remainder of the plate was covered with a precipitation of a redder colour. (fig. 2.) This thinner precipitation gave no indications of an increased substance as evidence of additional action at the points where the effects spreading from the two centres must be conceived to have been superimposed.

The plate having been once more cleaned, the double ball was placed nearly over its centre, and when connected with the galvanometer in circuit it affected it 55°. The single ball placed also nearly over the centre, but about two inches removed from the position of the double ball, affected it singly 45°. When the connexion was made with both the deflection increased to 57°. After four hours action with both the balls, a beautiful well-defined oval of pink copper precipitate was found immediately under

the two balls, of the diameter of 4 inches by 3½ nearly, beyond which there was a general diffusion of darker red precipitate (fig. 3.). The surface of this oval was perfectly smooth and compact, and presented no appearance of greater thickness or inequality at the centre than at any other part. The two balls seem to have acted as one oblong piece of generating metal would have done; and, upon the hypothesis of a radiant force from each, no increase was perceived at the points upon which the double set of rays might have been supposed to impinge.

I was desirous to repeat once more the experiments with the opposite sides of the conducting plate, and to connect them more closely with some of the previous series, by measuring the effects upon the calorific galvanometer, and by varying the form of the plate. For this purpose I took a circular plate of copper of the same diameter as the sphere, and immersed it in the copper solution. A zinc ball of 1⅛ inch diameter, placed in a membrane of acid over the centre, gave the following results:—

	First Series.	Second Series.
Ball at top . . .	79°	82°
Ball at middle . .	89	92
Ball at bottom . .	103	105

After these experiments the copper was found diffused over both sides of the plate, but did not extend to the centre of the under side.

The upper surface of the plate having been thoroughly covered with lac varnish, the experiment was repeated with the following results:—

Ball at top	69°
Ball at middle	75
Ball at bottom	70

The precipitated copper was here found deposited upon the under side in a ring about two inches in breadth from the edge, and there was very little in the centre of the plate, and none upon the upper surface.

I finally covered the under surface with varnish, leaving the upper exposed, with the following results:—

Ball at top	73°
Ball at middle	83
Ball at bottom	93

Hence it appears, that the under surface, which by itself is capable of sustaining an action from the ball in the centre of the solution nearly as great as the upper surface, when combined with the latter, adds no more than 10°, or about one-eighth to its efficiency. It appears also that, whereas with the upper surface, the action increases in some inverse ratio of the distance, of the generating from the conducting surface, with the under surface, there is a maximum point, on both sides of which it decreases.

This point is doubtless dependent upon the angle at which the force, which radiates (as it were) from the ball, meets the edge of the plate.

Wishing now to draw the preceding experiments into closer comparison with similar ones which had hitherto, generally, been made in a different form, I had a square glass cell constructed, measuring 3½ inches and four deep. This was cut in two in the middle, and the edges having been ground admitted of being clamped together, with the interposition of a piece of bladder. When thus put together it formed a cell divided into two by a diaphragm of membrane in the liquid, on either side of which different generating and conducting plates might be immersed. The forms which I selected were copper and zinc plates 3¼ inches square, and copper and zinc balls of one inch diameter: the results obtained by different combinations of these, at different distances apart, measured by the calorific galvanometer, are contained in the following Table. The electrolyte which was employed was, in one series, the standard acid on both sides of the diaphragm, and in the other the same acid in contact with the zinc, and the solution of copper with the copper. In the near distance the surfaces were about half an inch apart; in the farther three inches; and the balls were always placed opposite to the centre of the associated plate.

	Equal Plates.		Copper Plate and Zinc Ball.		Zinc Plate and Copper Ball.		Equal Balls.	
	Close.	Distant.	Close.	Distant.	Close.	Distant.	Close.	Distant.
Acid alone	15°	13°	11°	10°	6°	5°	—°	4°
Acid and Sol. Cop.	73	51	67	53	32	30	26	24

The precipitated copper in all these cases had been pretty evenly diffused over the near surface of the plates, and was to be traced to all parts of the opposite surface, but more upon the edges than towards the centres.

Upon this Table we may remark:—

1st. That the energy of the force was about sextupled by the absorption of the hydrogen upon the conducting surface, except in the case of the equal plates, when it was more than quadrupled.

2nd. That the effect of distance was much more decided in the instances where the amount of the circulating force was greater, than in the contrary cases.

3rd. That the amount of force put into circulation from a large surface of zinc towards a central ball of copper was, as in former instances of similar combinations, about one half of that from the reverse arrangement.

4th. That a ball of zinc exposing a surface of 3·14 square inches placed over the centre of a plate of copper exposing on its two sides a surface of twenty-eight square inches, sustained an action of nearly the same amount as a plate of zinc of the same dimensions as the copper, placed at the same distances.

This result, as well as the small effect produced in preceding experiments by substituting two equal balls, or a rod, for one ball of zinc, may, upon the supposition of

a force of the nature of a radiant force, be probably explained by the interference of the rays at points where their directions cross each other.

Let A B C D represent the sphere and *a* and *b* two active points within; the force radiates from *a* to *c* and A and *d*, and from *b* to *e* and *f* and C without interruption. But the rays proceeding from *a* to *g* and from *b* to *g*, encounter one another, and the force would appear to be directed in the diagonal of the two, or from *g* to B, and in a direction parallel to *a c* and *b c*.

A rod may obviously be considered as a succession of such balls or radiant points, and hence the force would be propagated in a direction at right angles to its axis towards a circumscribing sphere or cylinder.

Let *a b c d* represent the radiant points, then will the rays *a g* and *b g* interfere at *g* and pass on to *i*, *b f* and *c f* to B, and *c e*, *d e* to *l*. Or the same letters may represent radiant points in a plate A C, the rays of which will thus pass in a parallel direction to the opposite points of a conducting plate D E.

Without attaching, however, any importance to the geometrical diagrams, I would merely suggest that the resultants of all the radiant points acting from *a* to *d* towards D E may be parallel, and that such an hypothesis would account for the phenomena. The demonstration of this, if possible, would go far beyond my power in mathematical science.

The rays thus supposed to pass between two equal plates become parallel, and hence the decrease of the force will be directly as the distance, as Mr. Snow Harris found it by direct experiment.

Now nearly the whole of the preceding experiments, except those of the zinc sphere, had been made before I had the pleasure of reading the Eleventh of your Series of Experimental Researches on Electric Induction*; and I had been led to the supposition, which I believe I mentioned to you, that the force which is developed by voltaic combinations might be subject to the law of radiant forces; but I had been utterly at a loss to understand how, upon this hypothesis, it could extend its influence to the side of a plate opposite to that to which it was directed in right lines; how, in short (to make use of a term which you have happily employed to describe, what I now believe to be, a perfectly analogous phenomenon), it could "*turn a corner*." Since the perusal of that paper, however, everything seems to me to fall in so naturally with the general views which you have therein explained, that I almost feel as if I were intruding upon ground which is properly your own in venturing to apply the laws which you have established of the "Essential and Fundamental Principle of Induction"† to the explanation of some of the foregoing results. Supposing my views to be correct, you must have been led to them in the natural course of your investigations; and nothing in my own opinion could justify my interference in a work which must have been more completely performed by you, but the circumstance that I was led to it by the obvious direction of my own previous inquiries into voltaic combinations.

* Philosophical Transactions, 1838, p. 1.

† Ibid. p. 2.

The direction of the force through an electrolyte placed between a generating and conducting surface of two metals, may, I conceive, be expressed in the very words which you have employed to describe that of the *direct inductive force* in statical electricity.

"It may be conceived to be exerted in lines between the two limiting conducting surfaces, and is accompanied by a lateral or transverse force equivalent to a dilatation or repulsion of these representative lines; or the attractive force which exists amongst the particles of the electrolyte (dielectric) in the direction of the current (induction) is accompanied by a repulsive or diverging force in the transverse direction*."

The proof of this is exactly of the same nature as that which you have brought forward in the parallel instance of induction, namely, the turning round the corner of a plate; and I cannot but advert to the complete analogy of the case in which you brought your carrier ball near to the middle of a flat disc of metal placed upon an excited shell-lac cylinder when no charge was communicated, although one was obtained at the edge of the disc; and that of the deposition of a ring of precipitated copper round the edge of the under surface of a brass plate while the centre was free from it (fig. 1.).

This "lateral tension of the lines of force on one another" is quite consistent with their *divergence* from an active centre: may it not even be considered as the cause of their radiation? It is most particularly evidenced by the results of those experiments, in which the immediate divergence of the force from the active centre was prevented, by placing the latter in a glass tube, or by drawing it up above the general level of the surrounding electrolyte. In these instances the first impulse must have been propagated in a perpendicular direction; but the instant it was at liberty to influence the general mass, the molecules of the latter were thrown into the polarized state, and the direction of the force opened out as from a centre.

On the other hand, the same "repulsive force in a transverse direction" must be opposed to the *convergence* of the lines from an active sphere towards an interior conducting point, when the force is not stationary but current: may not this opposition account for the reduced action of a sphere of zinc upon a ball of copper? The difference of the *statical induction* and the *current induction* is, that in the former the force is not progressive, while in the latter it is in a state of perpetual flux; the state of polarity, however, and of tension, is maintained in both.

The transfer of the elements of the electrolyte in opposite directions under that peculiar molecular arrangement or polarity, "which is the first step in all electrolyzation," is quite compatible with their unequal distribution upon the limiting conducting surfaces, according to the varying relations of their dimensions and distances, as was evidenced by the unequal precipitation of the reduced copper in several of the preceding instances; but no correspondent inequality of the force can exist upon the surface of the conductors themselves, upon all parts of which it can instantaneously distribute itself with comparative facility.

* Philosophical Transactions, 1838, p. 37.

The principal circumstance which might be supposed to limit the power of an active point within a conducting sphere in any given electrolyte, is the resistance of that electrolyte, which increases in a certain ratio to its depth or thickness; and this thickness may be considered virtually the same, wherever the included point may be placed, but to increase with the diameter of the sphere. It is also the same, and consequently the resistance is also the same, when placed anywhere within the plane which divides the sphere into two hemispheres. But in an insulated hemisphere, the approximation of the active point to the lower surface virtually decreases the thickness of the electrolyte through which its action has to be propagated, by increasing the extent of surface which cuts the divergent lines of force; and consequently the force increases. In this respect the action of a point upon a plate may be considered the same as upon an indefinitely large hemisphere, towards which as the point approaches the force increases.

It appears to me that practical consequences of some importance flow from the preceding conclusions, upon which I may be tempted to address you again at no distant period.

I remain, my dear FARADAY,

Ever faithfully yours,

J. F. DANIELL.

King's College,
January 13, 1838.

III. *Of such Ellipsoids consisting of homogeneous matter as are capable of having the resultant of the attraction of the mass upon a particle in the surface, and a centrifugal force caused by revolving about one of the axes, made perpendicular to the surface.*

By James Ivory, *K.H. M.A. F.R.S. L. & E. Instit. Reg. Sc. Paris. Corresp. et Reg. Sc. Gottin. Corresp.*

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1. In the *Conn. des Temps* for 1837 it is announced that a homogeneous ellipsoid with three unequal axes, and consisting of particles that attract one another according to the law of nature, may be in equilibrium when it revolves with a proper velocity about the least axis. Lagrange has considered this problem in its utmost generality. The illustrious Geometer found the true equations from which the solution must be derived: but he inferred from them that a homogeneous planet cannot be in equilibrium unless it have a figure of revolution. Nevertheless M. Jacobi has proved that an equilibrium is possible in some ellipsoids of which the three axes have a certain relation to one another. The same thing is demonstrated by M. Liousville in 23rd cahier of the *Journal de l'École Polytechnique*. M. de Pontécoulant has also touched on the subject*. M. Jacobi has thus detected an inadvertence into which those had fallen who preceded him in this research. He has shown that the equations which, according to Lagrange, are capable of solution only in figures of revolution, may be solved in a certain class of ellipsoids with three unequal axes. But the transcendent equations of M. Jacobi, although fit for numerical computation on particular suppositions, leave unexplored the points of the problem which it is most interesting to know.

It is easy to find a property characteristical of all spheroids with which an equilibrium is possible on the supposition of a centrifugal force. From any point in the surface of the ellipsoid draw a perpendicular to the least axis, and likewise a line at right angles to the surface: if the plane passing through these two lines contain the resultant of the attractions of all the particles of the spheroid upon the point in the surface, the equilibrium will be possible; otherwise not. This will be evident, if it be considered that the resultant of the centrifugal force and the attraction of the mass must be a force perpendicular to the surface of the ellipsoid, which requires that the directions of the three forces shall be contained in one plane. This determination obviously comprehends all spheroids of revolution; but, on account of the

* Tom. iii. Théor. Anal.

complicated nature of the attractive force, it is difficult to deduce from it whether an equilibrium be possible, or not, in spheroids with three unequal axes.

The problem is unconnected with the physical conditions of equilibrium: it is purely a geometrical question respecting a property of certain ellipsoids.

2. Let the three semi-axes of an ellipsoid be represented by

$$k,\ k\sqrt{1+\lambda^2},\quad k\sqrt{1+\lambda'^2},$$

λ being supposed greater than λ'; and put x, y, z respectively parallel to the axes, for the coordinates drawn from a point in the surface to the principal sections of the solid: from the same point draw the line ϱ within the ellipsoid at right angles to its surface; and ϱ being limited by the principal section perpendicular to k, the axis of rotation, put p and q for the coordinates of the end of it in that plane, p being parallel to y, and q to z: from the condition that ϱ is perpendicular to the surface of the ellipsoid, it is easy to deduce the values of p and q, viz.

$$p = y \cdot \frac{\lambda^2}{1+\lambda^2},\quad q = z \cdot \frac{\lambda'^2}{1+\lambda'^2}.$$

Again, from the same point in the surface, draw the line ϱ' in the direction of the resultant of the attraction of the whole mass of the ellipsoid; and let r and s, respectively parallel to y and z, represent the coordinates of the foot of ϱ' in the same principal section as before: then ϱ' will be the diagonal of a parallelopiped of which the three sides are $x, y-r, z-s$; and the only three forces acting parallel to the sides of the parallelopiped and equivalent to the single force in the direction of the diagonal, will be proportional to the sides, $x, y-r, z-s$. Now from the nature of the ellipsoid, the attractive forces perpendicular to the principal sections, are proportional to the coordinates x, y, z; and may be represented by $\mathrm{A}\,x$, $\mathrm{B}\,y$, $\mathrm{C}\,z$: and, as these forces have their resultant in the direction of ϱ', it follows from what has been said, that they will be proportional to $x,\ y-r,\ z-s$. In consequence we have these equations,

$$\mathrm{A} = \frac{y}{y-r}\cdot \mathrm{B},\quad \mathrm{A} = \frac{z}{z-s}\cdot \mathrm{C};$$

$$r = y\,.\left(1-\frac{\mathrm{B}}{\mathrm{A}}\right),\quad s = z\,.\left(1-\frac{\mathrm{C}}{\mathrm{A}}\right):$$

and, by combining the values of r and s with those of p and q before found, we obtain

$$\frac{r-p}{s-q} = \frac{y}{z}\cdot\frac{\mathrm{B}-\frac{\mathrm{A}}{1+\lambda^2}}{\mathrm{C}-\frac{\mathrm{A}}{1+\lambda'^2}}.$$

Let σ denote the third side of the triangle which has ϱ and ϱ' for its other sides: then σ will represent the only force which, together with the attractive force ϱ', will produce a resultant in the direction of ϱ at right angles to the surface of the ellipsoid. Now σ cannot stand for a centrifugal force unless, in every position, it be invariably

parallel to the line $\sqrt{y^2 + z^2}$ drawn from the point in the surface of the ellipsoid at right angles to the axis of rotation; and this condition requires that the triangle of which the sides are σ, $r - p$, $s - q$, shall be similar to the triangle formed by the parallel lines $\sqrt{y^2 + z^2}$, y, z. From the similarity of the triangles, we deduce

$$\frac{r-p}{s-q} = \frac{y}{z};$$

and hence, in consequence of the last formula, we finally obtain,

$$B - \frac{A}{1+\lambda^2} = C - \frac{A}{1+\lambda'^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

Every ellipsoid which verifies this formula is capable of an equilibrium when it is made to revolve with a proper angular velocity about the least axis; for the line ϱ' representing the attraction upon a point in the surface, the line σ will represent a centrifugal force, both in quantity and direction; and the resultant of these two forces will be perpendicular to the surface of the ellipsoid.

The equation (1.) results immediately from the investigation of LAGRANGE, who concluded that it admits of solution only in spheroids of revolution, that is, when $\lambda = \lambda'$ and $B = C$. By expressing the functions A, B, C in elliptic integrals, M. JACOBI has found that the equation may be solved when the three axes have a certain relation. It is therefore demonstrated in general, that a certain class of ellipsoids with three unequal axes is susceptible of an equilibrium on the supposition of a centrifugal force; but it still remains to investigate the precise limits within which this extension of the problem is possible, and to determine the ellipsoid when the centrifugal force is given.

3. In order to solve the problem in the view now taken of it, we must have recourse to the equations of LAGRANGE, which contain all the necessary conditions. Let f denote the intensity of the centrifugal force at the distance equal to unit from the axis of rotation; the same force urging the point in the surface of the ellipsoid at the distance $\sqrt{y^2 + z^2}$ from the axis, will be equal to $f\sqrt{y^2 + z^2}$, the components of which in the directions of y and z are respectively fy and fz. Now

$$A\,x,\ B\,y,\ C\,z,$$

are the attractions of the mass of the ellipsoid; wherefore the total forces urging the point in the surface are

$$A\,x,\ (B - f)\,y,\ (C - f)\,z.$$

These forces must have their resultant in the direction of ϱ perpendicular to the surface of the ellipsoid; and as they are parallel to the sides of a parallelopiped, of which ϱ is the diagonal, they will be proportional to those sides, that is, to

$$x, \quad y - p = \frac{y}{1+\lambda'^2}, \quad z - q = \frac{z}{1+\lambda'^2}.$$

We thus obtain these two equations,

$$\frac{B-f}{A}=\frac{1}{1+\lambda^2},\qquad \frac{C-f}{A}=\frac{1}{1+\lambda'^2};$$

and from these we deduce

$$\left.\begin{aligned} f&=B-\frac{A}{1+\lambda^2},\\ f&=C-\frac{A}{1+\lambda'^2},\end{aligned}\right\}\quad\ldots\ldots\ldots\ldots\ldots\quad(2.)$$

which coincide with the equations of LAGRANGE.

It is next requisite to substitute for the symbols A, B, C, what they stand for. The values given in the *Mécanique Céleste* are in a convenient form for this purpose, viz.

$$dF=\frac{x^2\,dx}{\sqrt{(1+\lambda^2x^2)\cdot(1+\lambda'^2x^2)}},$$

$$A=\frac{3M}{k^3}\int_0^1 dF,\quad B=\frac{3M}{k^3}\int_0^1\frac{dF}{1+\lambda^2x^2},\quad C=\frac{3M}{k^3}\int_0^1\frac{dF}{1+\lambda'^2x^2}.$$

In these expressions M is the mass of the ellipsoid; therefore if we put ρ for the density, we shall have

$$\frac{M}{k^3}=\frac{4\pi\rho}{3}\cdot(1+\lambda^2)^{\frac{1}{2}}(1+\lambda'^2)^{\frac{1}{2}}.$$

These several values being substituted in the equations (2.), the result will be

$$\left.\begin{aligned} q&=\frac{f}{\frac{4\pi}{3}\rho},\\ q&=\sqrt{\frac{1+\lambda'^2}{1+\lambda^2}}\cdot\int_0^1\frac{\lambda^2\cdot 3x^2\,dx\,(1-x^2)}{(1+\lambda^2x^2)^{\frac{1}{2}}(1+\lambda'^2x^2)^{\frac{1}{2}}},\\ q&=\sqrt{\frac{1+\lambda^2}{1+\lambda'^2}}\cdot\int_0^1\frac{\lambda'^2\cdot 3x^2\,dx\,(1-x^2)}{(1+\lambda^2x^2)^{\frac{1}{2}}(1+\lambda'^2x^2)^{\frac{1}{2}}}.\end{aligned}\right\}\quad\ldots\ldots\ldots\quad(3.)$$

Here q stands for the proportion of the intensities of the centrifugal and attractive forces; it depends only on the kind of matter of which the spheroid is formed, and the velocity of rotation.

4. The equations (3.) comprehend all ellipsoids that are susceptible of an equilibrium on the supposition of a centrifugal force. To begin with the more simple case of the spheroid of revolution, let $\lambda=\lambda'=l$; and the two equations will coincide in one, viz.

$$q=\int_0^1\frac{l^2\cdot 3x^2(1-x^2)\,dx}{(1+l^2x^2)^2},\quad\ldots\ldots\ldots\ldots\quad(4.)$$

which expresses the relation between q and l, in a spheroid of revolution having its semi-axes equal to k and $k\sqrt{1+l^2}$.

From the equation (4.) we learn that q will be known when l is given, or that every spheroid of a determinate form requires an appropriate velocity of rotation.

The inspection of the same equation is sufficient to show that q is positive for all values of l^2; and as it vanishes both when l^2 is zero and infinitely great, it must pass at least once from increasing to decreasing, or it will admit of at least one maximum value. By differentiating with regard to l we obtain

$$\frac{dq}{2\,l\,dl} = \int_0^1 \frac{3\,x^2\,(1-x^2)\,(1-l^4\,x^2)}{(1+l^2\,x^2)^3}; \quad \ldots\ldots\ldots\ldots \quad (5.)$$

from which formula we learn that $\frac{dq}{2\,l\,dl}$ is positive between the limits $l^2 = 0$ and $l^2 = 1$; that it will consist of a positive and a negative part when l^2 is greater than 1; and the positive part decreasing while the negative part increases, that it will ultimately be negative when l^2 is infinitely great. It follows therefore that $\frac{dq}{2\,l\,dl}$ can be only once equal to zero, and consequently that q can have only one maximum value, while l^2 increases from 0 to ∞. Applying to the equations (4.) and (5.) the known method of integration, we get

$$q = \frac{3\,(3+l^2)}{2\,l^3} \text{ arc tan } l - \frac{9}{2\,l^2},$$

$$\frac{dq}{2\,l\,dl} = \frac{3\,(9+l^2)}{2\,l^4} \text{ arc tan } l - \frac{3\,l\,(9+7\,l^2)}{1+l^2},$$

of which expressions the first will verify the other. To determine the maximum of q, we have

$$\frac{dq}{2\,l\,dl} = 0$$

$$\text{arc tan } l = \frac{9\,l + 7\,l^3}{(1+l^2)\,(9+l^2)};$$

and the only value of l in this last equation is

$$l = 2{\cdot}5293.$$

By substituting this value of l we obtain 0·3370 for the maximum of q. With respect to spheroids of revolution it thus appears that an equilibrium is impossible when q or $\frac{f}{\frac{4\pi}{3}\rho}$ is greater than 0·3370: in the extreme case, when q is equal to 0·3370, there is only one form of equilibrium, the axes of the spheroid being

$$k \text{ and } k\sqrt{1+(2{\cdot}5293)^2} = 2{\cdot}7197\,k;$$

but when q is less than 0·3370 there are two different forms of equilibrium, the equatorial radius of one being less, and of the other greater, than 2·7197 k, k being the semi-axis of rotation.

The number of the forms of equilibrium in spheroids of revolution is purely a mathematical deduction from the expression of q; and as this has been known since the time of Maclaurin, the discussion of it was all that was wanted for perfecting this part of the theory.

5. Returning now to the general equations of the problem, let

$$\varphi = \int_0^1 3x^2\,dx \cdot \frac{\sqrt{(1+\lambda^2)\cdot(1+\lambda'^2)}}{\sqrt{(1+\lambda^2x^2)\cdot(1+\lambda'^2x^2)}};$$

and it will be found that the two equations (3.) are thus expressed:

$$q = \frac{d\varphi}{d\lambda}\lambda, \quad q = \frac{d\varphi}{d\lambda'}\lambda'.$$

Further, put $p = \lambda\lambda'$, $\tau^2 = (\lambda - \lambda')^2$, and we shall have

$$\varphi = \int_0^1 3x^2\,dx \cdot \frac{\sqrt{(1+p)^2+\tau^2}}{\sqrt{(1+px^2)^2+\tau^2x^2}};$$

and the two values of q in the partial differentials of φ relatively to λ and λ' being expressed in the partial differentials relatively to p and τ^2, we shall obtain

$$q = \frac{d\varphi}{d\lambda}\lambda = \frac{d\varphi}{dp}p + \frac{d\varphi}{\tau\,d\tau}(\lambda - \lambda')\lambda,$$

$$q = \frac{d\varphi}{d\lambda'}\lambda' = \frac{d\varphi}{dp}p - \frac{d\varphi}{\tau\,d\tau}(\lambda - \lambda')\lambda'.$$

These two values of q coalesce in one when $\lambda - \lambda' = 0$, that is, in spheroids of revolution; and we thus fall again upon the same equation that has already been discussed. In all other cases the two values cannot subsist together, unless

$$\left.\begin{aligned} q &= \frac{d\varphi}{dp}p, \\ 0 &= \frac{d\varphi}{\tau\,d\tau}, \end{aligned}\right\} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (6.)$$

which equations apply exclusively to ellipsoids with three unequal axes, and solve the problem with regard to that class. The latter of the equations (6.) expresses the relation that the two quantities p and τ^2 must have to one another in every ellipsoid with three unequal axes which is susceptible of an equilibrium. The fluxional operation indicated being performed in the same equation, the result will be,

$$0 = \int_0^1 \frac{x^2(1-x^2)(1-p^2x^2)\,dx}{\left((1+px^2)^2+\tau^2x^2\right)^{\frac{3}{2}}}, \quad \ldots\ldots\ldots\ldots\ldots (7.)$$

which is no other than a transformation of the equation (1.), and is equivalent to other transformations of the same equation found by M. Jacobi and M. Liouville.

The formula (7.) cannot be verified unless p, or $\lambda\lambda'$, be greater than 1; for if p were equal to 1, or less than 1, the integral would be positive. This agrees with the limitation of M. Jacobi.

If any value be assigned to τ^2, it is evident that a corresponding value of p may be found which will verify the formula (7.): for, if p be made to increase continually above 1, the integral, which is positive at first, will finally be negative; and it must be zero, in passing from one of these states to the other. This proves that there does exist an infinite number of ellipsoids not of revolution, which are susceptible of an equilibrium.

Let V stand for the integral in the equation (7.); and supposing that p and τ^2 vary so as always to satisfy that equation, we shall have

$$\frac{d\,\mathrm{V}}{dp}\,dp + \frac{d\,\mathrm{V}}{\tau\,d\tau}\,\tau\,d\tau = 0.$$

Now, τ^2 representing any positive quantity, we may conceive it to increase from zero to be infinitely great; in which case it follows from the nature of the function V, that during the whole increase $\frac{d\,\mathrm{V}}{\tau\,d\tau}\,\tau\,d\tau$ will be negative: wherefore the other term $\frac{d\,\mathrm{V}}{dp}\,dp$ will be positive; which requires that p decrease continually. Since p decreases when τ^2 increases, the greatest value of p will answer to the least value of τ^2, that is, to zero; and hence, by making $\tau^2 = 0$ in the formula (7.), we shall obtain this equation, viz.

$$0 = \int_0^1 \frac{x^3(1-x^2)(1-p^2x^2)\,dx}{(1+px^2)^3},$$

for finding the greatest value of p.

It is obvious that there is only one value of p that will verify the equation just found; for the integral can pass only once from being positive to be negative while p increases from 1 to be infinitely great. Let $p = l^2$, $lx = z$; and the equation will be changed into this which follows,

$$0 = \int_0^l \frac{dz\,(l^2z^3 - (1+l^4)z^4 + l^2z^6)}{(1+z^2)^3};$$

of which the integral is,

$$0 = l^2 z + \frac{(1+l^2)^2}{8}\cdot\frac{3z+5z^3}{(1+z^2)^2} - \frac{3+14l^2+3l^4}{8}\int_0^l \frac{dz}{1+z^2};$$

and hence, by making $z = l$, we deduce

$$\text{arc tan } l = \frac{3l+13l^3}{3+14l^2+3l^4}.$$

The only solution of this equation is $l = 1{\cdot}3934$; and $1{\cdot}9414$ is therefore the greatest value of $p = l^2$. Thus, in all the ellipsoids susceptible of an equilibrium by revolving about the least axis, $\lambda\lambda' = p$ is contained between the limits $1{\cdot}9414$ and 1, while $(\lambda - \lambda')^2 = \tau^2$, increases from zero to be infinitely great.

An elliptical spheroid formed of a homogeneous fluid, can be in equilibrium by the action of a centrifugal force, only when it revolves about the least axis. What has been said determines completely the series of ellipsoids with which an equilibrium is possible, when the three axes are unequal. Representing these axes by

$$k, \quad k\sqrt{1+\lambda^2}, \quad k\sqrt{1+\lambda'^2},$$

it has been shown that $\lambda\lambda'$ must be contained between the limits $1{\cdot}9414$ and 1, while $(\lambda - \lambda')^2$ varies from zero to be infinitely great. One limit is when $\lambda = \lambda'$, being a spheroid of revolution of which the axes are

$$k \text{ and } k\sqrt{2{\cdot}9414} = k \times 1{\cdot}7150.$$

Supposing λ and λ' to vary from this extreme, when the first increases, the other will decrease; so that, when λ is infinitely great, λ' will be zero; which proves that the other extreme limit is a cylinder extending indefinitely on either side of the base, which is a circle having k for its radius.

6. It remains to consider the value of q. In the first of the equations (6.), let the operation indicated be performed, and the result will be

$$q = \int_0^1 \frac{p \cdot 3x^2(1-x^2)\,dx \cdot \{(1+p)(1+px^2)+p\tau^2x^2\}}{((1+p)^2+\tau^2)^{\frac{1}{2}} \cdot ((1+px^2)^2+\tau^2x^2)^{\frac{1}{2}}};$$

and from this we obtain the value of q in the extreme case when $\tau^2 = 0$, or when λ and λ' are equal, viz.

$$q = \int_0^1 \frac{p \cdot 3x^2(1-x^2)\,dx}{(1+px^2)^2},$$

which is no other than the determination of q in a spheroid of revolution having its axes equal to

$$k \text{ and } k\sqrt{2{\cdot}9414} = k \times 1{\cdot}7150.$$

In the other extreme case, when τ^2 is infinitely great, q is zero.

It has been shown that for every given value of τ^2, there is only one value of p, and only one ellipsoid; and when τ^2 and p are both ascertained, the foregoing expression proves that q is fully determined. Thus there is an appropriate value of q to every ellipsoid susceptible of an equilibrium.

In the formula for q, one of the two quantities, τ^2 and p, increases when the other decreases; and hence it may be surmised that more than one ellipsoid may answer to a given value of q. Some calculation is necessary to elucidate this point. For the sake of abridging expressions, put

$$\mathrm{P} = \sqrt{(1+p)^2+\tau^2}$$
$$\mathrm{Q} = \sqrt{(1+px^2)^2+\tau^2x^2}$$
$$\mathrm{M} = p(1+p)(1+px^2)+p^2\tau^2x^2$$
$$du = 3x^2(1-x^2)\,dx,$$

the variation of du being between the limits $x = 0$ and $x = 1$: then, the foregoing value of q will be thus written:

$$q = \int \frac{du \cdot \mathrm{M}}{\mathrm{P}\mathrm{Q}^3};$$

and, q being considered a function of p and τ^2, the fluxion with respect to p will be

$$\frac{dq}{dp} = \int \frac{du}{\mathrm{P}\mathrm{Q}^3} \cdot \left\{ \frac{d\mathrm{M}}{dp} - \frac{(1+p)\mathrm{M}}{\mathrm{P}^2} - \frac{3x^2(1+px^2)\mathrm{M}}{\mathrm{Q}^2} \right\};$$

it will be found that

$$\frac{(1+p)\mathrm{M}}{\mathrm{P}^2} = p(1+px^2) - \frac{\tau^2p(1-p^2x^2)}{\mathrm{P}^2},$$

$$\frac{3x^2(1+px^2)\mathrm{M}}{\mathrm{Q}^2} = 3px^2(1+p) - \frac{3\tau^2px^4(1-p^2x^2)}{\mathrm{Q}^2};$$

wherefore

$$\frac{dq}{dp} = \left(\frac{1}{P} + \frac{\tau^2 p}{P^3}\right) \cdot \int \frac{du\,(1-p^2x^2)}{Q^3} + \frac{p}{P}\int du\,(1-x^2)$$
$$+ \frac{p\tau^2}{P} \cdot \int du \cdot \frac{2\,Q^2x^2 + 3x^4 - 3p^2x^6}{Q^5};$$

in consequence of the formula (7.) the first term is zero, so that we have

$$\frac{dq}{dp} = \frac{p}{P}\int du\,(1-x^2) + \frac{p\tau^2}{P}\int du \cdot \frac{2x^2 + (3+4p)x^4 - p^2x^6 + 2\tau^2x^4}{Q^5}.$$

And because $3 + 4p$ is always greater than p^2, it follows that $\frac{dq}{dp}$ is essentially positive.

Again, by taking the fluxion relatively to τ^2, we have

$$\frac{dq}{\tau\,d\tau} = \int \frac{du}{P\,Q^3} \cdot \left\{\frac{p^3x^2\,P^2 - M}{P^2} + \frac{p^3x^2\,Q^2 - 3x^2\,M}{Q^2}\right\};$$

that is,

$$\frac{dq}{\tau\,d\tau} = -\frac{p+p^3}{P^3}\int du \cdot \frac{1-p^2x^2}{Q^3}$$
$$- \frac{p}{P}\int du \cdot \frac{(3+2p)x^2 + (3p+p^3)x^4 - p^3x^6 + 2p\tau^2x^2}{Q^5};$$

Of this value the first term is zero by the formula (7.); and attending to the limits of p and of the integral, the second term is essentially negative.

Now we have

$$dq = \frac{dq}{dp} \cdot dp + \frac{dq}{\tau\,d\tau} \cdot \tau\,d\tau;$$

if we suppose τ^2 to increase, p will decrease; and according to what has been shown the two parts of dq will be negative. Wherefore, while τ^2 increases from zero to be infinitely great, q will decrease continually from its first value to zero; and for every possible value of q there will be only one value of τ^2, and consequently only one ellipsoid susceptible of an equilibrium.

It would be superfluous to pursue this investigation further, and a mere waste of labour to seek the easiest formulas for solving a problem which, it appears from what has been shown, can have no application in the theory of the figure of the planets. It is extremely probable that no such figures as those required for the equilibrium of ellipsoids with three unequal axes, will be found to exist in nature. It seems difficult to admit that any circumstances, or the action of any forces we are acquainted with, could induce upon a mass of fluid a figure adjusted with such mathematical nicety to the attraction of the mass and the centrifugal force. If the existence of such a figure can be supposed, would it be permanent? Would not the least action of the other bodies of the system upon it be sufficient to destroy the exact confor-

mation on which the equilibrium depends, and leave the fluid to adjust its figure solely by the attraction and the centrifugal force of its particles? The discovery of JACOBI makes no change in the usual theory of the figure of the planets; but it is valuable, as it completes a mathematical speculation, and finally settles what relates to the figure of ellipsoids susceptible of an equilibrium.

IV. *Researches towards establishing a Theory of the Dispersion of Light. No. IV.*

By the Rev. Baden Powell, *M.A. F.R.S. F.G.S., Savilian Professor of Geometry in the University of Oxford.*

Received January 11,—Read February 8, 1838.

Introductory Remarks.

In my last communication I laid before the Royal Society a comparison of the results of observation and of theory, with respect to the dispersion of light, in the instances of the refractive indices for the standard rays in fifteen different cases of transparent media (some being the same medium at different temperatures), including those which exhibit the greatest range, and the highest numbers, of any yet subjected to this kind of observation. The agreement with the theory was found to be sufficiently close for the lower cases, but displayed an increasing discrepancy as we advanced towards the higher. The theoretical formula employed was one derived from the undulatory hypothesis, by a process involving some limitations, which rendered it only approximative; and, in conclusion, I remarked that by pursuing the investigation to a greater degree of development, or by adopting methods of a more precise character, it was still reasonably to be hoped that a more close coincidence might be found.

I alluded specifically to the methods of M. Cauchy and of Mr. Kelland, as those to which we might look for the means of following up the inquiry with good prospect of success. Of the former (delivered in the Nouveaux Exercices de Mathématiques, Prague, 1835–6, and extending through livraisons 1 to 8 inclusive), I can only say that the investigations are of so extremely elaborate a character, that I was glad, in the first instance at least, to try any other method which might seem to promise results without involving calculations of such overwhelming extent as those by which the distinguished author establishes the exact agreement with theory of all the indices observed by Fraunhofer.

I therefore commenced with a trial of the method proposed in the memoir of Mr. Kelland*, applying it of course in the first instance to the case of the most highly dispersive substance, oil of cassia, in which the greatest discrepancy had before appeared. Owing to an obscurity in the statement of an important part of the process in the paper referred to, I was led to communicate with the author, and

* Cambridge Transactions, vol. vi. Part I.

soon received from him a statement of the results of theory for oil of cassia, in which the discrepancies were almost wholly removed.

I have since verified that calculation, and have performed similar computations for the only other cases in which material differences before appeared.

The object of the present communication is to state these results, with the necessary data of the calculations; and further, to elucidate the general method, so as to render it more readily applicable to other cases which may arise in the further prosecution of the determination of refractive indices; and to notice the present condition in which the theory may be considered to stand with respect to this material portion of its experimental evidence.

Explanation of the Formula.

The formula adopted in my preceding papers includes essentially the development of the term

$$\frac{\sin\left(\frac{\theta}{\lambda}\right)}{\left(\frac{\theta}{\lambda}\right)}.$$

This of course gives a series involving the *even* powers of λ with certain coefficients. And the practical differences in the *methods of calculation* turn entirely on the number of terms to which it may be thought necessary to pursue this series, or the mode of finding or eliminating the coefficients.

As it does not enter into my present design to refer to the *physical* principles of the theory, I will merely here observe, that though such principles have been assumed under some difference of aspect by the several eminent mathematicians who have treated the subject, yet the formulas deduced for the dispersion have, in every instance, resulted the same as far as the *form* of the series is concerned, differing only in regard to the nature of the summation and the coefficients involved.

As it is in regard to the numerical comparison with experiment that I am at present engaged in considering the subject, I have been chiefly interested in comparing these methods so far as to see whether, when one might fail in giving sufficiently close coincidences, another might cause the discrepancies to diminish or disappear.

In this view then, referring to Mr. KELLAND's method, it may be necessary for its better elucidation to state it *generally* as follows. Supposing it sufficient to take three terms of the series, the relation of the refractive index (μ) to the wave-length *in* the medium (λ_1) may be expressed thus:

$$\frac{1}{\mu^2} = p - \frac{1}{\lambda_1^2} q + \frac{1}{\lambda_1^4} l.$$

Our comparison, however, is to be made with the wave-length in air or vacuum, which, in order to express that in the medium, must be reduced in the ratio of the refraction for the medium and for the ray, (which is not expressed in the author's formulas,) or, λ being the wave-length in air, we must take

$$\lambda_1 = \frac{\lambda}{\mu},$$

and the formula becomes

$$\frac{1}{\mu^2} = p - \left(\frac{\mu}{\lambda}\right)^2 q + \left(\frac{\mu}{\lambda}\right)^4 l$$

taking such formulas successively for the different standard rays, between any two, as those for B and E, the constant p is eliminated: and combining these with a third, as that for H, the coefficients q and l are determined. For brevity writing

$$\frac{1}{\mu^2_B} = b, \quad \left(\frac{\mu}{\lambda}\right)^2_B = \beta, \quad \left(\frac{\mu}{\lambda}\right)^4_B = \beta^2;$$

and similarly expressing by $e, \varepsilon, \varepsilon^2$; h, η, η^2; the corresponding quantities for the rays E and H, we shall have

$$(b - e) = (\varepsilon - \beta)\, q - (\varepsilon^2 - \beta^2)\, l,$$
$$(e - h) = (\eta - \varepsilon)\, q - (\eta^2 - \varepsilon^2)\, l;$$

whence we obtain,

$$l = \frac{(\varepsilon - \beta)(e - h) - (\eta - \varepsilon)(b - e)}{(\varepsilon - \beta)(\eta^2 - \varepsilon^2) - (\eta - \varepsilon)(\varepsilon^2 - \beta^2)},$$

$$q = \frac{(b - e) + (\varepsilon^2 - \beta^2)\, l}{(\varepsilon - \beta)}.$$

Knowing the values of λ from the determinations of FRAUNHOFER, it becomes easy in the above formula to introduce the values of $\left(\frac{\mu}{\lambda}\right)$ taking the indices as given by observation for the particular medium: we, thus, first determine the constants q and l for the medium, and having done this, by the aid of these combined again with the indices given by observation a value of p is deduced for each ray by the formula,

$$p = \frac{1}{\mu^2} + \left(\frac{\mu}{\lambda}\right)^2 q - \left(\frac{\mu}{\lambda}\right)^4 l;$$

and if these values of p for the different rays result *equal*, the theory is verified.

Mr. KELLAND has thus verified it to a degree of accuracy, which will probably be deemed sufficient, for all the indices determined by FRAUNHOFER.

The following Table contains the logarithms of the values of $\frac{1}{\lambda^2}$ for the standard rays after the determinations of FRAUNHOFER, without their index.

Ray	$\log \frac{1}{\lambda^2}$.
B	·18999
C	·23165
D	·32508
E	·42216
F	·48236
G	·59885
H	·66892

In order to simplify the numerical calculations, it is found convenient to regard the two last terms of the formula as involving factors which are respectively some power of ten in the numerators, to the same amount as the number of places which would be found in the values of λ^2 and λ^4 in the denominators.

In applying the theory to the cases of particular media, we have to combine the values in the above Table with those of the indices obtained from observation. These I have taken from my own approximate determinations, as originally given in a separate memoir, and quoted in my last paper in the Philosophical Transactions, 1837, Part I.

In the following cases therefore the logarithms of λ^2 are taken as above, and after deriving those of $\left(\frac{\mu}{\lambda}\right)^2$ and of $\left(\frac{\mu}{\lambda}\right)^4$ a common index 4 is added: from these we obtain in the first instance the values of q and l, and thence again those of p for each ray.

I have not, in the present instance, thought it necessary to go through these somewhat laborious calculations for more than those three cases which in my former investigations appeared to present the greatest discrepancies with theory, viz. the oil of cassia, which gave the greatest discordances; and the two sets of observations on sulphuret of carbon at the respective temperatures of 12° and 22° centigrade.

Comparison of observed refractive indices with the results of Mr. KELLAND'S *theory.*

I. Sulphuret of Carbon. Temp. 12°.

Ray.	Log. μ^2 from obs.	Values of $\frac{1}{\mu^2}$ from obs.	$\left(\frac{\mu}{\lambda}\right)^2 q$.	$\left(\frac{\mu}{\lambda}\right)^4 l$.	p.
B	·42084	·37946	·01461	·00031	·39438
C	·42214	·37832	·01612	·00037	·39481
D	·42804	·37322	·02027	·00059	·39408
E	·43476	·36749	·02573	·00096	·39418
F	·44106	·36220	·03000	·00131	·39351
G	·45392	·35163	·04041	·00237	·39441
H	·46614	·34187	·04884	·00352	·39423

$\log l = \bar{9}\cdot27073$

$\log q = \bar{7}\cdot55372$

II. Sulphuret of Carbon. Temp. 22°.

B	·41408	·38470	·01784	·00029	·40225
C	·41774	·38217	·01979	·00036	·40160
D	·42288	·37768	·02484	·00057	·40195
E	·42996	·37157	·03157	·00090	·40224
F	·43608	·36637	·03678	·00125	·40190
G	·44878	·35588	·04953	·00227	·40314
H	·46136	·34565	·05922	·00332	·40225

$\log (-l) = \bar{9}\cdot26050$

$\log q = \bar{7}\cdot64725$

III. Oil of Cassia.

Ray.	Log μ^2 from obs.	Values of $\frac{1}{\mu^2}$ from obs.	$\left(\frac{\rho}{\lambda}\right)^2 q$.	$\left(\frac{\rho}{\lambda}\right)^4 l$.	p.
B	·40197	·39630	·01163	·00200	·40993
C	·40378	·39466	·01285	·00245	·40996
D	·40916	·38980	·01614	·00386	·40980
E	·41661	·38316	·02053	·00624	·40993
F	·42411	·37661	·02399	·00852	·40912
G	·44058	·36259	·03259	·01572	·41090
H	·46100	·34594	·04013	·02384	·40991

$\log l = \bar{8}\cdot11760$

$\log q = \bar{7}\cdot47363$

Observations on the above Results.

In the case of oil of cassia the accordance in the values of p appears sufficiently close; especially considering that the experimental data can only be regarded as approximations, as fully appears from my paper on the determination of the indices. The only material discrepancy is in the ray G; and it is this ray for which Mr. KELLAND himself has always found theory in excess in the calculation of FRAUNHOFER'S indices, and has made some remarks on the point in his memoir. Upon the whole, considering this as the extreme case as yet known and examined, the superiority of Mr. KELLAND'S method will be sufficiently manifest; and it will be allowed that this extreme case has been thus brought as far at least within the limits of accordance as we can perhaps reasonably expect in the present state of our means of investigation.

The case of sulphuret of carbon at the temperature of 12° is also brought into very satisfactory agreement with theory by the present method.

The other case of the same substance at the temperature of 22° still exhibits some discordance. The ray G is here again in excess; but the differences follow no regular order, being sometimes in excess, sometimes in defect. This at least shows that although the series is not rapidly convergent, in this case the addition of another term would not remove the discordance.

With regard to the error which is always found so marked in the ray G, Mr. KELLAND in a letter to me, observed that in that ray it would seem reasonable to entertain some suspicion as to the experimental data. Now there is one circumstance which may corroborate such suspicion. The determinations of the values of λ, as is well known, were made from the interference-spectrum, in which the blue end, with its dark lines, is most *contracted*. In the refraction-spectra, (and more so in the more dispersed,) it is the most *expanded*: and the dark bands which in the lower cases appear single, in the higher are *resolved* into several lines, in some instances separated by very sensible intervals: and this difference must be still more marked in comparing the highly dispersed spectra with that of interference. The ray G, in par-

ticular, is thus resolved into an assemblage of small lines. Thus some uncertainty may be fairly admitted to exist in the data; at any rate enough to render further examination desirable before we can pronounce on the insufficiency of the theory.

General Remarks on the Formula.

If the accordances be allowed to come sufficiently within the limits of error, it may not be improper to add a remark with respect to the entire nature of the formula, and the light in which, (in its present state,) the theory of dispersion must be regarded.

The relation here expressed between the index and the wave-length involves three constants dependent on the medium; which must be in some way derived from experimental data: and which are here directly deduced by assuming some three, at least, of the observed refractive indices for the medium.

The whole process then seems equivalent to assuming these three indices, and then interpolating the intermediate values. This, though under a different form, is also palpably the case with the method adopted in my former paper.

Now it may be contended that this actually carries us but a very little way towards a real or satisfactory explanation, and that a complete theory ought to assign also an independent relation between the constants.

The consideration of this point has been included in the valuable researches lately made by Professor Lloyd of Dublin, given in a paper read before the Royal Irish Academy, and noticed in the reports of that body, (Nos. 2 and 3.). But I have been informed by the author that, in pursuing that research, he has found theory, as yet, incapable of furnishing the relation in question.

It seems, therefore, that in the present state of our knowledge we must be content to regard the *constants* of the formula as unexplained by theory. But the process by which we here obtain them, (viz. by assuming three indices from observation,) may be viewed as simply *auxiliary*. The main calculation may be regarded as independent, and considered to involve two of these constants only *as if* they had been adopted *empirically*; whence we proceed to verify the formula by the coincidences of the values of the third, viz. p. But even with this deficiency, it seems to me not an unimportant step to be able, with two empirical constants, dependent on the medium, but independent of the ray, to assign a third quantity, which expresses for each ray a relation between the wave-length and the refractive index, with so near an approximation to the truth, even in the most extreme case as yet known.

Oxford, January 7, 1838.

V. *On the Colours of Mixed Plates.* By Sir David Brewster, *K.G.H. F.R.S.*

Received October 25,—Read December 14, 1837.

THE colours of mixed plates were discovered by Dr. Thomas Young*, and described in the Philosophical Transactions for 1802. He produced them by interposing small portions of water, or butter, or tallow between two plates of glass, or two object glasses pressed together so as to give the ordinary colours of thin plates. In this way portions or cavities of air were surrounded with water, butter, or tallow; and on looking through this combination of media he saw fringes or rings of colour six times larger than those of thin plates that would have been produced had air alone been interposed between the glasses. These fringes or rings of colour were seen by the direct light of a candle, and began from a white centre like those produced by transmission; but on the dark space next the edge of the plate, Dr. Young observed another set of fringes or rings, complementary to the first, and beginning from a black centre like those produced by reflection. This last set of colours was always brighter than the first.

The following is Dr. Young's explanation of these two series of colours.

"In order to understand," says he, "this circumstance, we must consider that where a dark object is placed behind the glasses, the whole of the light which comes to the eye is either refracted through the edges of the drops, or reflected from the internal surface; while the light which passes through those parts which are on the side opposite to the dark object consists of rays refracted as before through the edges, or simply passing through the fluid. The respective combinations of these portions of light exhibit a series of colours of different orders, since the internal reflection modifies the interference of the rays on the dark side of the object, in the same manner as in the common colours of thin plates seen by reflection. When no dark object is near, both these series of colours are produced at once; and since they are always of an opposite nature at any given thickness of a plate, they neutralize each other and constitute white light†."

In so far as I know, these observations have not been repeated by any other philosopher; and subsequent authors have only copied Dr. Young's description of the phenomena and acquiesced in his explanation of them. In taking up this subject I

* Since this paper was written I find that this class of colours was discovered by M. Mazeas, and that his experiments were repeated and varied by M. Dutour.

† Philosophical Transactions, 1802. Dr. Young republished the same explanation of mixed plates in 1807 in his Elements of Natural Philosophy. See vol. i. p. 470, 787; vol. ii. 635, 680.

never doubted the accuracy or the generality of the results obtained by so distinguished a philosopher. I was induced to study the phenomena of mixed plates as auxiliary to a more general inquiry; and having observed new phenomena of colour in mineral bodies, which have the same origin as those of mixed plates, and which lead to conclusions different from those of Dr. YOUNG, I am anxious that they should be described in the same work which contains his original observations.

Having experienced considerable difficulty in obtaining satisfactory specimens of the colours of mixed plates by using the substances employed by Dr. YOUNG, I sought for a method of producing them which should be at once easy and infallible in its effects. With this view I tried transparent soap, and whipped cream, which gave tolerably good results; but I obtained the best effect by using the white of an egg beat up into froth. To obtain a proper film of this substance I place a small quantity between the two glasses, and having pressed it out into a film I separate the glasses, and by holding them near the fire I drive off a little of the superfluous moisture. The two glasses are again placed in contact, and when pressed together so as to produce the coloured fringes or rings, they are then kept in their place either by screws or by wax, and may be preserved for any length of time.

If we now examine with a magnifier of small power the thin film of albumen, we shall find that it contains thousands of cavities exactly resembling the strata of cavities which I have described as occurring in topaz, quartz, sulphate of lime and other minerals*; and if we look through the film at the margin of the flame of a candle, we shall perceive the two sets of colours described by Dr. YOUNG, the one upon the luminous edge of the flame, and the other on the dark space contiguous to it. The first we shall call the *direct*, and the second, which are always the brightest, the *complementary fringes*.

If we apply a higher magnifying power to the albuminous films, and bring the edge of one of the cavities to the margin of the flame, we shall perceive that both the *direct* and the *complementary* colours are formed at the very edge, the complementary ones appearing just when the direct ones have disappeared, by the withdrawal of the edge from the flame.

As the colours therefore are produced solely by the edges of the cavities, their intensity must, *cæteris paribus*, depend on the smallness of the cavities, or the number of edges which occur in a given space. When we succeed in forming an uniform film in which the cavities are like a number of minute points, the phenomena are peculiarly splendid and we are enabled to study them with greater facility. When the edges of these cavities are seen by an achromatic microscope, and in direct light, neither the direct nor the complementary colours are visible; but if we gradually withdraw the lens from the cavities a series of beautiful phenomena appear. When the vision first becomes indistinct both the direct and the complementary colours appear at the same time, specks of the *complementary red* alternating with brighter

* Edin. Trans. vol. x. Part I. 407.

specks of the *direct green* light. By increasing the distance of the lens from the cavities, the complementary specks become less and less visible, and we see only the direct green light.

In order to study these phenomena by observing the action of a *single* edge upon light, and to ascertain the effect of an edge when there were no prismatic edges to refract, and no internal surface to reflect light, I conceived the idea of immersing thin plates of a solid substance in a fluid of such a refractive power, that the thickness of the plates should be virtually reduced to the same degree of thinness as the film of albumen between the plates of glass. The new substance described by Mr. HORNER*, and which I shall call *nacrite*, furnished me with the means of performing this experiment. I accordingly inclosed the thinnest films of it between two plates of glass containing balsam of capivi; and I had the satisfaction of observing that the bounding edge of the plate and the fluid produced the identical direct and complementary colours above described.

The bounding edge which I selected for observation gave a *bright green* for the *direct*, and a *bright red* for the *complementary* tint. This edge appeared as a narrow distinct black line, exceedingly well defined, and of a uniform breadth like the finest micrometer wire. It consequently obstructed the incident light and produced the phenomena of diffracted fringes. These fringes, however, were modified by the peculiar circumstances under which they were produced, and exhibited in their tints both the direct and complementary colours under consideration.

When the diffracted fringes are viewed in candle-light by a lens placed at a greater distance from the diffracting edge than its principal focus, the middle of the system of fringes corresponding to the diffracted shadow of a fibre is occupied with the *direct tint*, which we shall suppose to be *green*; and on each side of this *green* shadow, as we may call it, we observe very faintly the *complementary red* tinging what are called the two first exterior fringes. This tinge of red is strongest in the first fringe within the solid edge, or within the green shadow, while it is *yellowish* in the first fringe without the green shadow. These effects are inverted if we place the lens nearer to the edge than its principal focus.

The phenomena now described appear more distinct if we take an extremely narrow piece of nacrite, having its two edges nearly in contact, and transmitting only a narrow line of light. In this case the two red fringes within the solid edge unite their tints, and become a bright red; and in like manner if we place the lens nearer the solid edges than its principal focus, the two yellow fringes will unite their tints, and become a brighter yellow band. In this last case, when the two bounding edges are still nearer each other, the united fringes, in place of being yellow, will be *green*, or the same as the direct colour.

If we bring the edges of two pieces of nacrite of equal thickness very near each other, having, as formerly, *green* for the *direct*, and *red* for the *complementary* colour,

* Philosophical Transactions, 1836, p. 49.

the space between the edges, or between the green bands, will be faint *red* when the lens is nearer the edges than its principal focus, and *yellow* when it is further from them; but if the edges are brought still nearer, the faint red will become brighter, and the united green bands will take the place of the yellow one.

Let us now return to our plate of *nacrite* with a single edge, having *green* and *red* for the two tints; and let us always suppose that the lens is adjusted to observe the diffracted fringes, that is, that the lens is placed at a greater distance from the diffracting edge than its principal focus. We shall also suppose that the light of the sun passing through a narrow aperture parallel to the diffracting edge is substituted for the light of a candle. Under these circumstances the central part of the system of fringes seen by light incident perpendicularly, consists of *blue**, *green*, and *yellow* light, constituting, as it were, the shadow of the edge, the blue light being on the same side as the plate of nacrite, and the yellow rays encroaching upon the exterior faint red band already described, the other red band next the blue being more distinctly seen. If we now incline the incident ray to the plate of nacrite more than 90°, the faint red band next the yellow gradually becomes brighter, while the other bands become fainter; and at the boundary of light and darkness all the other bands disappear except this *red* one, which is the *complementary* colour to the *green*, (produced by the union of the *blue*, *green* and *yellow* bands,) and the colour which is seen upon the dark space next the edge of the flame, as described by Dr. Young. If we, on the other hand, incline the incident ray in an opposite direction, so that it forms with the plane of the plate a less angle than 90°, the *red* band next the blue will now become brighter; and at the boundary of light and darkness, when all the other bands have disappeared, the *red* band will afford the complementary colour to the *green*.

As the edge of the plate of nacrite is rough and unpolished, and accurately perpendicular to the parallel faces, there are no reflected nor refracted pencils, whose combinations with one another, or with the direct rays, can be employed to account for the complementary colours. The phenomena of mixed plates, indeed, are cases of diffraction when the light is obstructed by the edge of very thin transparent plates placed in a medium of different refractive power. If the plate were opake the fringes would be exactly those which have been so often described, and explained by the principle of interference. But owing to the *transparency* of the plate, fringes are produced within its shadow; and owing to the *thinness* of the plate the light transmitted through it and retarded, interferes with the partial waves which pass through the plate and with those which pass beyond the diffracting edge with undiminished velocity, and modifies the usual system of fringes in the manner which we have described.

As the plate of nacrite diminishes in thickness, or as the fluid in which it is immersed approaches to it in refractive density, the central coloured bands, whose union constitutes the *direct* tint, will diminish in number, and descending gradually in the scale will finally disappear when the retardation produced by the plate does not per-

* Owing to the small quantity of blue rays in candle-light the blue almost disappears in it.

ceptibly alter the phase of the ray. When the plate, on the other hand, increases in thickness, or the fluid diminishes in refractive power, the central bands will become closer and more numerous, and will finally resemble the fringes within the shadow of the ordinary system.

When the plate of nacrite is thicker at one place than another by the partial removal of a parallel film, the edge where the increase of thickness takes place produces exactly the same phenomena as the edge of the film that is removed, or of the film that is elevated above the general surface, and hence we are led to look for the phenomena of mixed plates in minerals, such as *sulphate of lime* and *mica*, where a plate of two different thicknesses can be easily obtained. I have accordingly discovered the phenomena of mixed plates distinctly exhibited in sulphate of lime and mica.

A more splendid exhibition of these colours is seen when a stratum of cavities of extreme thinness occurs in sulphate of lime. I have observed such strata repeatedly in the gypsum from Mont-martre; but they are most beautiful when the stratum has a circular form. In this case the cavities are exceedingly thin at the circumference of the circle, and gradually increase in depth towards the centre, so that we have a series of edges increasing in thickness towards a centre; the very reverse of a mixed plate, such as a film of albumen pressed between two convex surfaces. The system of rings is therefore also reversed, the highest order of colours being in the centre, while the lowest are at the circumference of the circular stratum. In many strata of cavities, such as the one which I have engraven in my paper on the new fluids in minerals*, the cavities are too deep to give the colours of mixed plates.

Another example of the colours of mixed plates in natural bodies occurs in specimens of mica, through which titanium is disseminated in beautiful flat dendritic crystals of various degrees of opacity and transparency. In these specimens the titanium is often disseminated in grains, forming an irregular surface. The edges of these grains, by retarding the light which they transmit, produce the direct and complementary colours of mixed plates in the most perfect manner, the tints passing through two orders of colours, as the grains of titanium increase in size towards the interior of the irregular patch. I have observed another example of these colours in the deep cavities of topaz, from which the fluids have either escaped, leaving one or both of the surfaces covered with minute particles of transparent matter, or in which the fluids have suffered induration.

Allerly by Melrose,
October 18th, 1837.

* Edinburgh Transactions, vol. x. Plate II. fig. 33.

VI. *Supplementary Note to Experimental Researches in Electricity.—Eleventh Series.* By MICHAEL FARADAY, *Esq. D.C.L. F.R.S., Fullerian Prof. Chem. Royal Institution, Corr. Memb. Royal and Imp. Acadd. of Sciences, Paris, Petersburgh, Florence, Copenhagen, Berlin, &c. &c.*

Received March 29, 1838.

1307. I HAVE recently put into an experimental form that general statement of the question of *specific inductive capacity* which is given at No. 1252 of Series XI., and the result is such as to lead me to hope the Council will authorise its addition to the paper in the form of a supplementary note. Three circular brass plates, about five inches in diameter, were mounted side by side upon insulating pillars; the middle one, A, was a fixture, but the outer plates B and C were moveable on slides, so that all three could be brought with their sides almost into contact, or separated to any required distance. Two gold leaves were suspended in a glass jar from insulated wires; one of the outer plates B was connected with one of the gold leaves, and the other outer plate with the other leaf. The outer plates B and C were adjusted at the distance of an inch and a quarter from the middle plate A, and the gold leaves were fixed at two inches apart; A was then slightly charged with electricity, and the plates B and C, with their gold leaves, thrown out of insulation *at the same time*, and then left insulated. In this state of things A was charged positive inductrically, and B with C negative inducteously; the same dielectric, air, being in the two intervals, and the gold leaves hanging, of course, parallel to each other in a relatively unelectrified state.

1308. A plate of shell-lac three quarters of an inch in thickness, and four inches square, suspended by clean white silk thread, was very carefully deprived of all charge (1203.), so that it produced no effect on the gold leaves if A were uncharged, and then introduced between plates A and B; the electric relation of the three plates was immediately altered, and the gold leaves attracted each other. On removing the shell-lac this attraction ceased; on introducing it between A and C it was renewed; on removing it the attraction again ceased; and the shell-lac when examined by a delicate COULOMB electrometer was still without charge.

1309. As A was positive, B and C were of course negative; but as the specific inductive capacity of shell-lac is about twice that of air (1270.), it was expected that when the lac was introduced between A and B, A would induce more towards B than towards C; that therefore B would become more negative than before towards A, and

consequently, because of its insulated condition, be positive externally, as at its back or at the gold leaves; whilst C would be less negative towards A, and therefore negative outwards or at the gold leaves. This was found to be the case; for on whichever side of A the shell-lac was introduced the external plate at that side was positive, and the external plate on the other side negative towards each other, and also to uninsulated external bodies.

1310. On employing a plate of sulphur instead of shell-lac, the same results were obtained; consistent with the conclusions drawn regarding the high specific inductive capacity of that body already given (1276.).

1311. These effects of specific inductive capacity can be exalted in various ways, and it is this capability which makes the great value of the apparatus. Thus I introduced the shell-lac between A and B, and then for a moment connected B and C, uninsulated them, and finally left them in the insulated state; the gold leaves were of course hanging parallel to each other. On removing the shell-lac the gold leaves attracted each other; on introducing the shell-lac between A and C this attraction was *increased*, (as had been anticipated from theory,) and the leaves came together, though not more than four inches long, and hanging three inches apart.

1312. By simply bringing the gold leaves nearer to each other I was able to show the difference of specific inductive capacity when only thin plates of shell-lac were used, the rest of the dielectric space being filled with air. By bringing B and C nearer to A another great increase of sensibility was made. By enlarging the size of the plates still further power was gained. By diminishing the extent of the wires, &c. connected with the gold leaves, another improvement resulted. So that in fact the gold leaves became, in this manner, as delicate a test of *specific inductive action* as they are, in Bennet's and Singer's electrometers, of ordinary electrical charge.

1313. It is evident that by making the three plates the sides of cells, with proper precautions as regards insulation, &c., this apparatus may be used in the examination of gases, with far more effect than the former apparatus (1187. 1290.), and may, perhaps, bring out differences which have as yet escaped me (1292. 1293.).

1314. It is also evident that two metal plates are quite sufficient to form the instrument; the state of the single inducteous plate when the dielectric is changed, being examined either by bringing a body excited in a known manner towards its gold leaves, or, what I think will be better, employing a carrier ball in place of the leaf, and examining that ball by the Coulomb electrometer (1180.). The inductive and inducteous surfaces may even be balls; the latter being itself the carrier ball of the Coulomb's electrometer (1181. 1229.).

1315. To increase the effect, a small condenser may be used with great advantage. Thus if, when two inducteous plates are used, a little condenser were put in the place of the gold leaves, I have no doubt the three principal plates might be reduced to an inch or even half an inch in diameter. Even the gold leaves act to each other for

the time as the plates of a condenser. If only two plates were used, by the proper application of the condenser the same reduction might take place. This expectation is fully justified by an effect already observed and described (1229.).

1316. In that case the application of the instrument to very extensive research is evident. Comparatively small masses of dielectrics could be examined, as diamonds and crystals. An expectation, that the specific inductive capacity of crystals will vary in different directions, according as the lines of inductive force (1304.) are parallel to, or in other positions in relation to the axes of the crystals, can be tested: I purpose that these and many other thoughts which arise respecting specific inductive action and the polarity of the particles of dielectric matter, shall be put to the proof as soon as I can find time.

1317. Hoping that this apparatus will form an instrument of considerable use, I beg to propose for it (at the suggestion of a friend) the name of *Differential Inductometer*.

Royal Institution,
March 29th, 1838.

VII. *Experimental Researches in Electricity.—Twelfth Series.* By MICHAEL FARADAY, *Esq., D.C.L. F.R.S. Fullerian Prof. Chem. Royal Institution, Corr. Memb. Royal and Imp. Acadd. of Sciences, Paris, Petersburgh, Florence, Copenhagen, Berlin, &c. &c.*

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§. 18. *On Induction (continued).* ¶ vii. *Conduction, or conductive discharge.* ¶ viii. *Electrolytic discharge.* ¶ ix. *Disruptive discharge—Insulation—Spark—Brush—Difference of discharge at the positive and negative surfaces of conductors.*

1318. I PROCEED now, according to my promise, to examine, by the great facts of electrical science, that theory of induction which I have ventured to put forth (1165. 1295. &c.). The principle of induction is so universal that it pervades all electrical phenomena; but the general case which I purpose at present to go into consists of insulation traced into and terminating with discharge, with the accompanying effects. This case includes the various *modes* of discharge, and also the condition and characters of a current; the elements of magnetic action being amongst the latter. I shall necessarily have occasion to speak theoretically, and even hypothetically; and though these papers profess to be experimental researches, I hope that, considering the facts and investigations contained in the last series in support of the particular view advanced, I shall not be considered as taking too much liberty on the present occasion, or as departing too far from the character which they ought to have, especially as I shall use every opportunity which presents itself of returning to that strong test of truth, experiment.

1319. Induction has as yet been considered in these papers only in cases of insulation;—opposed to insulation is *discharge*. The action or effect which may be expressed by the general term *discharge*, may take place, as far as we are aware at present, in several modes. Thus, that which is called simply *conduction* involves no chemical action, and apparently no displacement of the particles concerned. A second mode may be called *electrolytic discharge*; in it chemical action does occur, and particles must, to a certain degree, be displaced. A third mode, namely, that by sparks or brushes, may, because of its violent displacement of the particles of the *dielectric* in its course, be called the *disruptive discharge*; and a fourth may, perhaps, be conveniently distinguished for a time by the words *convection*, or *carrying discharge*, being that in which discharge is effected either by the carrying power of solid par-

ticles, or those of gases and liquids. Hereafter, perhaps, all these modes may appear as the result of one common principle, but at present they require to be considered apart; and I will now speak of the *first* mode, for amongst all the forms of discharge that which we express by the term conduction appears the most simple and the most directly in contrast with insulation.

¶ vii. *Conduction, or conductive discharge.*

1320. Though assumed to be essentially different, yet neither CAVENDISH nor POISSON attempt to explain by, or even state in, their theories, what the essential difference between insulation and conduction is. Nor have I anything, perhaps, to offer in this respect, *except* that, according to my view of induction, both it and conduction depend upon the same molecular action of the dielectrics concerned; are only extreme degrees of *one common condition* or effect; and in any sufficient mathematical theory of electricity must be taken as cases of the same kind. Hence the importance of the endeavour to show the connection between them under my theory of the electrical relations of contiguous particles.

1321. Though the action of the insulating dielectric in the charged Leyden jar, and that of the wire in discharging it, may seem very different, they may be associated by numerous intermediate links, which carry us on from one to the other, leaving, I think, no necessary connection unsupplied. We may observe some of these in succession for information respecting the whole case.

1322. Spermaceti has been examined and found to be a dielectric, through which induction can take place (1240. 1246.), its specific inductive capacity being about or above 1·8 (1279.), and the inductive action has been considered in it, as in all other substances, an action of contiguous particles.

1323. But spermaceti is also a *conductor*, though in so low a degree that we can trace the process of conduction, as it were, step by step through the mass (1247.); and even when the electric force has travelled through it to a certain distance, we can, by removing the coercitive (which is at the same time the inductive) force, cause it to return upon its path and reappear in its first place (1245. 1246.). Here induction appears to be a necessary preliminary to conduction. It of itself brings the contiguous particles of the dielectric into a certain condition, which, if retained by them, constitutes *insulation*, but if lowered by the communication of power from one particle to another, constitutes *conduction*.

1324. If *glass* or *shell-lac* be the substances under consideration, the same capabilities of suffering either induction or conduction through them appear (1233. 1239. 1247.), but not in the same degree. The conduction almost disappears (1239. 1242.); the induction therefore is sustained, i. e. the polarized state into which the inductive force has brought the contiguous particles is retained, there being little discharge action between them, and therefore the *insulation* continues. But, what discharge there is, appears to be consequent upon that condition of the particles into

which the induction throws them; and thus it is that ordinary insulation and conduction are closely associated together, or rather are extreme cases of one common condition.

1325. In ice or water we have a better conductor than spermaceti, and the phenomena of induction and insulation therefore quickly disappear, because conduction quickly follows upon the assumption of the inductive state. But let a plate of cold ice have metallic coatings on its sides, and connect one of these with a good electrical machine in work, and the other with the ground, and it then becomes easy to observe the phenomena of induction through the ice, by the electrical tension which can be obtained and continued on both the coatings (419. 426.). For although that portion of power which at one moment gave the inductive condition to the particles is at the next lowered by the consequent discharge due to the conductive act, it is succeeded by another portion of force from the machine to restore the inductive state. If the ice be converted into water, the same succession of actions can be just as easily proved, provided the water be distilled, and (if the machine be not powerful enough) a voltaic battery be employed.

1326. All these considerations impress my mind strongly with the conviction, that insulation and ordinary conduction cannot be properly separated when we are examining into their nature; that is, into the general law or laws under which their phenomena are produced. They appear to me to consist in an action of contiguous particles dependent on the forces developed in electrical excitement; these forces bring the particles into a state of tension or polarity, which constitutes both *induction* and *insulation*; and being in this state, the continuous particles have a power or capability of communicating their forces one to the other, by which they are lowered, and discharge occurs. Every body appears to discharge (444); but the possession of this capability in a *greater or smaller degree* in different bodies, makes them better or worse conductors, worse or better insulators; and both *induction* and *conduction* appear to be the same in their principle and action (1320.), except that in the latter an effect common to both is raised to the highest degree, whereas in the former it occurs in the best cases, in only an almost insensible quantity.

1327. That in our attempts to penetrate into the nature of electrical action, and to deduce laws more general than those we are at present acquainted with, we should endeavour to bring apparently opposite effects to stand side by side in harmonious arrangement, is an opinion of long standing, and sanctioned by the ablest philosophers. I hope, therefore, I may be excused the attempt to look at the highest cases of conduction as analogous to, or even the same in kind with, those of induction and insulation.

1328. If we consider the slight penetration of sulphur (1241. 1242.) or shell-lac (1234.) by electricity, or the feebler insulation sustained by spermaceti (1279. 1240.), as essential consequences and indications of their *conducting* power, then may we look on the resistance of metallic wires to the passage of electricity through them as

insulating power. Of the numerous well known cases fitted to show this resistance in what are called the perfect conductors, the experiments of Professor WHEATSTONE best serve my present purpose, since they were carried to such an extent as to show that *time* entered as an element into the conditions of conduction* even in metals. When discharge was made through a copper wire 2640 feet in length, and $\frac{1}{11}$th of an inch in diameter, so that the luminous sparks at each end of the wire, and at the middle, could be observed in the same place, the latter was found to be sensibly behind the two former in time, they being by the conditions of the experiment, simultaneous. Hence a proof of retardation; and what reason can be given why this retardation should not be of the same kind as that in spermaceti, or in lac, or sulphur? But as, in them, retardation is insulation, and insulation is induction, why should we refuse the same relation to the same exhibitions of force in the metals?

1329. We learn from the experiment, that if *time* be allowed the retardation is gradually overcome; and the same thing obtains for the spermaceti, the lac, and glass; give but time in proportion to the retardation, and the latter is at last vanquished. But if that be the case, and all the results are alike in kind, the only difference being in the length of time, why should we refuse to metals the previous inductive action, which is admitted to occur in the other bodies? The diminution of *time* is no negation of the action; nor is the lower degree of tension requisite to cause the forces to traverse the metal, as compared to that necessary in the cases of water, spermaceti, or lac. These differences would only point to the conclusion, that in metals the particles under induction can transfer their forces when at a lower degree of tension or polarity, and with greater facility than in the instances of the other bodies.

1330. Let us look at Mr. WHEATSTONE's beautiful experiment in another point of view. If, leaving the arrangement at the middle and two ends of the long copper wire unaltered, we remove the two intervening portions and replace them by wires of iron or platina, we shall have a much greater retardation of the middle spark than before. If, removing the iron, we were to substitute for it only five or six feet of water in a cylinder of the same diameter as the metal, we should have still greater retardation. If from water we passed to spermaceti, either directly or by gradual steps through other bodies, (even though we might vastly enlarge the bulk, for the purpose of evading the occurrence of a spark elsewhere (1331.) than at the three proper intervals,) we should have still greater retardation, until at last we might arrive, by degrees so small as to be inseparable from each other, at actual and permanent insulation. What, then, is to separate the principle of these two extremes, perfect conduction and perfect insulation, from each other; since the moment we leave in the smallest degree perfection at either extremity, we involve the element of perfection at the opposite end? Especially too, as we have not in nature the case of perfection either at one extremity or the other, either of insulation or conduction.

* Philosophical Transactions, 1834, p. 583.

1331. Again, to return to this beautiful experiment in the various forms which may be given to it: the forces are not all in the wire (after they have left the Leyden jar) during the whole time (1328.) occupied by the discharge; they are disposed in part through the surrounding dielectric under the well-known form of induction; and if that dielectric be air, induction takes place from the wire through the air to surrounding conductors, until the ends of the wire are electrically related through its length and discharge has occurred, i. e. for the *time* during which the middle spark is retarded beyond the others. This is well shown by the old experiment, in which a long wire is so bent that two parts (Plate III. fig. 1. *a*. *b*.) near its extremities shall approach within a short distance, as a quarter of an inch, of each other in the air. If the discharge of a Leyden jar, charged to a sufficient degree, be sent through such a wire, by far the largest portion of the electricity will pass as a spark across the air at the interval, and not by the metal. Does not the middle part of the wire, therefore, act here as an insulating medium, though it be of metal? and is not the spark through the air an indication of the tension (simultaneous with *induction*) of the electricity in the ends of this single wire? Why should not the wire and the air both be regarded as dielectrics; and the action at its commencement, and whilst there is tension, as an inductive action? If it acts through the contorted lines of the wire, so it also does in curved lines through air (1219. 1224.), and other insulating dielectrics (1228.); and we can apparently go so far in the analogy, whilst limiting the case to the inductive action only, as to show that amongst insulating dielectrics some lead away the lines of force from others (1229.), as the wire will do from worse conductors, though in it the principal effect is no doubt due to the ready discharge between the particles whilst in a low state of tension. The retardation is for the time insulation; and it seems to me we may just as fairly compare the air at the interval *a*, *b*, (fig. 1.) and the wire in the circuit, as two bodies of the same kind and acting upon the same principles, as far as the first inductive phenomena are concerned, notwithstanding the different forms of discharge which ultimately follow*, as we may compare, according to Coulomb's investigations†, *different lengths* of different insulating bodies required to produce the same amount of insulating effect.

1332. This comparison is still more striking when we take into consideration the experiment of Mr. Harris, in which he stretched a fine wire across a glass globe, the air within being rarefied‡. On sending a charge through the joint arrangement of metal and rare air, as much, if not more, electricity passed by the latter as by the former. In the air, rarefied as it was, there can be no doubt the discharge was preceded by induction (1284.); and to my mind all the circumstances indicate that the same was the case with the metal; that, in fact, both substances are dielectrics, ex-

* These will be examined hereafter (1348, &c.).

† Mémoires de l'Académie, 1785, p. 612. or Ency. Britann. First Supp. vol. i. p. 611.

‡ Philosophical Transactions, 1834, p. 242.

hibiting the same effects in consequence of the action of the same causes, the only variation being one of degree in the different substances employed.

1333. Judging on these principles, velocity of discharge through the *same wire* may be varied greatly by attending to the circumstances which cause variations of discharge through spermaceti or sulphur. Thus, for instance, it must vary with the tension or intensity of the first urging force (1234. 1240.), which tension is charge and induction. So if the two ends of the wire, in Professor WHEATSTONE's experiment, were immediately connected with two large insulated metallic surfaces exposed to the air, so that the primary act of induction, after making the contact for discharge, might be in part removed from the internal portion of the wire at the first instant, and disposed for the moment on its surface jointly with the air and surrounding conductors, then I venture to anticipate that the middle spark would be more retarded than before; and if these two plates were the inner and outer coating of a large jar or a Leyden battery, then the retardation of that spark would be still greater.

1334. CAVENDISH was perhaps the first to show distinctly that discharge was not always by one channel*, but, if several are present, by many at once. We may make these different channels of different bodies, and by proportioning their thicknesses and lengths, may include such substances as air, lac, spermaceti, water, protoxide of iron, iron and silver, and by *one* discharge make each convey its proportion of the electric force. Perhaps the air ought to be excepted, as its discharge by conduction is questionable at present; but the others may all be limited in their mode of discharge to pure conduction. Yet several of them suffer previous induction, precisely like the induction through the air, it being a necessary preliminary to their discharging action. How can we therefore separate any one of these bodies from the others, as to the *principles and mode* of insulating and conducting, except by mere degree? All seem to me to be dielectrics acting alike, and under the same common laws.

1335. I might draw another argument in favour of the general sameness, in nature and action, of good and bad conductors (and all the bodies I refer to are conductors more or less), from the perfect equipoise in action of very different bodies when opposed to each other in magneto-electric inductive action, as formerly described (213.), but am anxious to be as brief as is consistent with the clear examination of the probable truth of my views.

1336. With regard to the possession by the gases of any conducting power of the simple kind now under consideration, the question is a very difficult one to determine at present. Experiments seem to indicate that they do insulate certain low degrees of tension perfectly, and that the effects which may have appeared to be occasioned by *conduction* have been the result of the carrying power of the charged particles,

* Philosophical Transactions, 1776, p. 197.

either of the air or of dust, in it. It is equally certain, however, that with higher degrees of tension or charge they discharge to one another, and that is conduction. If they possess the power of insulating a certain low degree of tension continuously and perfectly, such a result may be due to their peculiar physical state, and the condition of separation under which their particles are placed. But in that, or in any case, we must not forget the fine experiments of CAGNIARD DE LA TOUR*, in which he has shown that liquids and their vapours can be made to pass gradually into each other, to the entire removal of any marked distinction of the two states. Thus, hot dry steam and cold water pass by insensible gradations into each other; yet the one is amongst the gases as an insulator, and the other a comparatively good conductor. As to conducting power, therefore, the transition from metals even up to gases is gradual; substances make but one series in this respect, and the various cases must come under one condition and law (444.). The specific differences of bodies as to conducting power only serves to strengthen the general argument, that conduction, like insulation, is a result of induction, and is an action of contiguous particles.

1337. I might go on now to consider induction and its concomitant, *conduction*, through mixed dielectrics, as, for instance, when a charged body, instead of acting across air to a distant uninsulated conductor, acts jointly through it and an interposed insulated conductor. In such a case, the air and the conducting body are the mixed dielectrics; and the latter assumes a polarized condition as a mass, like that which my theory assumes *each particle* of the air to possess at the same time. But I fear to be tedious in the present condition of the subject, and hasten to the consideration of other matter.

1338. To sum up, in some degree, what has been said, I look upon the first effect of an excited body upon neighbouring matters to be the production of a polarized state of their particles, which constitutes *induction*; and this arises from its action upon the particles in immediate contact with it, which again act upon those contiguous to them, and thus the forces are transferred to a distance. If the induction remain undiminished, then perfect insulation is the consequence; and the higher the polarized condition which the particles can acquire or maintain, the higher is the intensity which may be given to the acting forces. If, on the contrary, the contiguous particles, upon acquiring the polarized state, have the power to communicate their forces, then conduction occurs, and the tension is lowered, conduction being a distinct act of discharge between neighbouring particles. The lower the state of tension at which this discharge between the particles of a body takes place, the better conductor is that body. In this view, insulators may be said to be bodies whose particles can retain the polarized state; whilst conductors are those whose particles cannot be permanently polarized. If I be right in my view of induction, then I consider the reduction of these two effects (which have been so long held distinct) to an action of

* Annales de Chimie, xxi. pp. 127. 178; or Quarterly Journal of Science, xv. 145.

contiguous particles obedient to one common law, as a very important result; and, on the other hand, the identity of character which the two acquire when viewed by the theory (1326.), is additional presumptive proof in favour of the correctness of the latter.

1339. That heat has great influence over simple conduction is well known (445.), its effect being, in some cases, almost an entire change of the characters of the body (432. 1340.). HARRIS has, however, shown that it in no respect affects gaseous bodies, or at least air*; and DAVY has taught us that, as a class, metals have their conducting power *diminished* by it†.

1340. I formerly described a substance, sulphuret of silver, whose conducting power was increased by heat (433. 437. 438.); and I have since then met with another as strongly affected in the same way: this is fluoride of lead. When a piece of that substance, which had been fused and cooled, was introduced into the circuit of a voltaic battery, it stopped the current. Being heated, it acquired conducting powers before it was visibly red hot in daylight; and even sparks could be taken against it whilst still solid. The current alone then raised its temperature (as in the case of sulphuret of silver) until it fused, after which it seemed to conduct as well as the metallic vessel containing it; for whether the wire used to complete the circuit touched the fused fluoride only, or was in contact with the platina on which it was supported, no sensible difference in the current was observed. During all the time there was scarcely a trace of decomposing action on the fluoride, and what did occur, seemed referable to the air and moisture of the atmosphere, and not to electrolytic action.

1341. I have now very little doubt that periodide of mercury (414. 448. 691.) is a case of the same kind, and also corrosive sublimate (692.). I am also inclined to think, since making the above experiments, that the anomalous action of the protoxide of antimony, formerly observed and described (693. 801.), may be referred in part to the same cause.

1342. I have no intention at present of going into the particular relation of heat and electricity, but we may hope hereafter to discover by experiment the law which probably holds together all the above effects with those of the *evolution* and the *disappearance* of heat by the current, and the striking and beautiful results of thermo-electricity, in one common bond.

¶ viii. *Electrolytic discharge.*

1343. I have already expressed in a former paper (1164.) the view by which I hope to associate ordinary induction and electrolyzation. Under that view, the discharge of electric forces by electrolyzation is rather an effect superadded, in a certain class

* Philosophical Transactions, 1834, p. 230. † Ibid. 1821, p. 431.

of bodies, to those already described as constituting induction and insulation, than one independent of and distinct from these phenomena.

1344. Electrolytes, as respects their insulating and conducting forces, belong to the general category of bodies (1320. 1334.); and if they are in the solid state (as nearly all can assume that state), they retain their place, presenting then no new phenomenon (426, &c.); or if one occur being in so small a proportion as to be almost unimportant. When liquefied, they also belong to the same list whilst the electric intensity is below a certain degree; but at a given intensity (910. 912. 1007.), fixed for each, and very low in all known cases, they play a new part, causing discharge in proportion (783.) to the development of certain chemical effects of combination and decomposition; and at this point, move out from the general class of insulators and conductors, to form a distinct one by themselves. The former phenomena have been considered (1320. 1338.); it is the latter which have now to be revised, and used as a test of the proposed theory of induction.

1345. The theory assumes, that the particles of the dielectric (now an electrolyte) are in the first instance brought, by ordinary inductive action, into a polarized state, and raised to a certain degree of tension or intensity before discharge commences; the inductive state being, in fact, a *necessary preliminary* to discharge. By taking advantage of those circumstances which bear upon the point, it is not difficult to increase the tension indicative of this state of induction, and so make the state itself more evident. Thus, if distilled water be employed, and a long narrow portion of it placed between the electrodes of a powerful voltaic battery, we have at once indications of the intensity which can be sustained at these electrodes by the inductive action through the water as a dielectric, for sparks may be obtained, gold leaves diverged, and Leyden bottles charged at their wires. The water is in the condition of the spermaceti (1322. 1323.), a bad conductor and a bad insulator; but what it does insulate is by virtue of inductive action, and that induction is the preparation for and precursor of discharge (1338.).

1346. The induction and tension which appear at the limits of the portion of water in the direction of the current, are only the sums of the induction and tension of the contiguous particles between those limits; and the limitation of the inductive tension, to a certain degree shows (time entering in each case as an important element of the result), that when the particles have acquired a certain relative state, *discharge*, or a transfer of forces equivalent to ordinary conduction, takes place.

1347. In the inductive condition assumed by water before discharge comes on, the particles polarized are the particles of the *water*, that being the dielectric used; but the discharge between particle and particle is not, as before, a mere interchange of their powers or forces at the polar parts, but an actual separation of them into their two elementary particles, the oxygen travelling in one direction, and carrying with it its amount of the force it had acquired during the polarization, and the hydrogen doing the same thing in the other direction, until they each meet the next approaching

particle, which is in the same electrical state with that they have left, and by association of their forces with it, produce what constitutes discharge. This part of the action may be regarded as a carrying one (1319.), performed by the constituent particles of the dielectric. The latter is always a compound body (664. 823.); and by those who have considered the subject and are acquainted with the philosophical view of transfer which was first put forth by GROTTHUSS*, its particles may easily be compared to a series of metallic conductors under inductive action, which, whilst in that state, are divisible into these elementary moveable halves.

1348. Electrolytic discharge depends, of necessity, upon the non-conduction of the dielectric as a whole, and there are two steps or acts in the process: first a polarization of the molecules of the substance, and then a lowering of the forces by the separation, advance in opposite directions, and recombination of the elements of the molecules, they being, as it were, the halves of the originally polarized conductors or particles.

1349. These views of the decomposition of electrolytes and the consequent effect of discharge, which, as to the particular case, are the same with those of GROTTHUSS (481.) and DAVY (482.), though they differ from those of BIOT (487.), DE LA RIVE (490.), and others, seem to me to be fully in accordance not merely with the theory I have given of induction generally (1165.), but with all the known *facts* of common induction, conduction, and electrolytic discharge; and in that respect help to confirm, in my mind, the truth of the theory set forth. The new mode of discharge which electrolyzation presents must surely be an evidence of the *action of contiguous particles*; and as this appears to depend directly upon a previous inductive state, which is the same with common induction, it greatly strengthens the argument which refers induction in all cases to an action of contiguous particles also (1295, &c.).

1350. As an illustration of the condition of the polarized particles in a dielectric under induction, I may describe an experiment. Put into a glass vessel some clear rectified oil of turpentine, and introduce two wires passing through glass tubes where they are at the surface of the fluid, and terminating either in balls or points. Cut some very clean dry white silk into small particles, and put these also into the liquid; then electrify one of the wires by an ordinary machine and discharge by the other. The silk will immediately gather from all parts of the liquid, and form a band of particles reaching from wire to wire, and if touched by a glass rod will show considerable tenacity; yet the moment the supply of electricity ceases, the band will fall away and disappear by the dispersion of its parts. The *conduction* by the silk is in this case very small; and after the best examination I could give to the effects, the impression on my mind is, that the adhesion of the whole is due to the polarity which each filament acquires, exactly as the particles of iron between the poles of a horse-shoe magnet are held together in one mass by a similar disposition of forces. The particles of silk therefore represent to me the condition of the molecules of the dielectric itself, which I assume to be polar, just as that of the silk is. In all cases of conductive dis-

* Annales de Chimie, lviii. 60. and lxiii. 20.

charge the contiguous polarized particles of the body are able to effect a neutralization of their forces with greater or less facility, as the silk does also in a very slight degree. Further we are not able to carry the parallel, except in imagination; but if we could divide each particle of silk into two halves, and let each half travel until it met and united with the next half in an opposite state, it would then exert its carrying power (1347.), and so far represent electrolytic discharge.

1351. Admitting that electrolytic discharge is a consequence of previous induction, then how evidently do its numerous cases point to induction in curved lines (1216.), and to the divergence or lateral action of the lines of inductive force (1231.), and so strengthen that part of the general argument in the former paper! If two balls of platina, forming the electrodes of a voltaic battery, are put into a large vessel of dilute sulphuric acid, the whole of the surfaces are covered with the respective gases in beautifully regulated proportions, and the mind has no difficulty in conceiving the direction of the curved lines of discharge, and even the intensity of force of the different lines, by the quantity of gas evolved upon the different parts of the surface. Hence the general effects of diffusion; the appearance of the anions or cathions round the edges and on the further side of the electrodes when in the form of plates; the manner in which the current or discharge will follow all the forms of the electrolyte, however contorted. Hence the effects which Nobili has so well examined and described* in his papers on the distribution of currents in conducting masses. All these effects indicate the direction of the currents or discharges which occur in and through the dielectrics, and these are in every case *preceded* by equivalent inductive actions of the contiguous particles.

1352. Hence also the advantage, when the exciting forces are weak or require assistance, of enlarging the mass of the electrolyte; of increasing the size of the electrodes; of making the coppers surround the zincs:—all is in harmony with the view of induction which I am endeavouring to examine; I do not perceive as yet one fact against it.

1353. There are many points of *electrolytic discharge* which ultimately will require to be very closely considered, though I can but slightly touch upon them. It is not that, as far as I have investigated them, they present any contradiction to the view taken (for I have carefully, though unsuccessfully, sought for such cases), but simply want of time as yet to pursue the inquiry, which prevents me from entering upon them here.

1354. One point is, that different electrolytes or dielectrics require different initial intensities for their decomposition (912.). This may depend upon the degree of polarization which the particles require before electrolytic discharge commences. It is in direct relation to the chemical affinity of the substances concerned; and will probably be found to have a relation or analogy to the specific inductive capacity of different bodies (1252. 1296.). It thus promises to assist in causing the great truths

* Bibliotheque Universelle, 1835, lix. 263. 416.

of those extensive sciences, which are occupied in considering the forces of the particles of matter, to fall into much closer order and arrangement than they have heretofore presented.

1355. Another point is, the facilitation of electrolytic conducting power or discharge by the addition of substances to the dielectric employed. This effect is strikingly shown where water is the body whose qualities are improved, but, as yet, no general law governing all the phenomena has been detected. Thus some acids, as the sulphuric, phosphoric, oxalic, and nitric, increase the power of water enormously; whilst others, as the tartaric and citric acids, give but little power; and others, again, as the acetic and boracic acids, do not produce a change sensible to the voltameter (739.). Ammonia produces no effect, but its carbonate does. The caustic alkalies and their carbonates produce a fair effect. Sulphate of soda, nitre (753.), and many soluble salts produce much effect. Percyanide of mercury and corrosive sublimate produce no effect; nor does iodine, gum, or sugar, the test being a voltameter. In many cases the added substance is acted on either directly or indirectly, and then the phenomena are more complicated; such substances are muriatic acid (758.), the soluble protochlorides, (766.), and iodides (769.), nitric acid (752.), &c. In other cases the substance added is not, when alone, subject to or a conductor of the powers of the voltaic battery, and yet both gives and receives power when associated with water. M. DE LA RIVE has pointed this result out in sulphurous acid*, iodine and bromine†; the chloride of arsenic produces the same effect. A far more striking case, however, is presented by that very influential body sulphuric acid (681.); and probably phosphoric acid also is in the same peculiar relation.

1356. It would seem in the cases of those bodies which suffer no change themselves, as sulphuric acid (and perhaps in all), that they affect water in its conducting power only as an electrolyte; for whether little or much improved, the decomposition is proportionate to the quantity of electricity passing (727. 730.), and the transfer is therefore due to electrolytic discharge. This is in accordance with the fact already stated as regards water (984.), that the conducting power is not improved for electricity of force below the electrolytic intensity of the substance acting as the dielectric; but both facts (and some others) are against the opinion which I formerly gave, that the power of salts, &c. might depend upon their assumption of the liquid state by solution in the water employed (410.). It occurs to me that the effect may perhaps be related to, and have its explanation in differences of specific inductive capacities.

1357. I have described in the last paper, cases, where shell-lac was rendered a conductor by absorption of ammonia (1294.). The same effect happens with muriatic acid; yet both these substances, when gaseous, are non-conductors; and the

* Quarterly Journal, xxvii. 407. or Bibliotheque Universelle, xl. 205. KEMP says sulphurous acid is a very good conductor, Quarterly Journal, 1831, p. 613.

† Quarterly Journal, xxiv. 465. or Annales de Chimie, xxxv. 161.

ammonia, also when in strong solution (748.). Mr. HARRIS has mentioned instances* in which the conducting power of metals is seriously altered by a very little alloy. These may have no relation to the former cases, but nevertheless should not be overlooked in the general investigation which the whole question requires.

1358. Nothing is perhaps more striking in that class of dielectrics which we call electrolytes, than the extraordinary and almost complete suspension of their peculiar mode of effecting discharge when they are rendered *solid* (380, &c.), even though the intensity of the induction acting through them may be increased a hundred fold or more (419.). It not only establishes a very general relation between the physical properties of these bodies and electricity acting by induction through them, but draws both their physical and chemical relations so near together, as to make us hope we shall shortly arrive at the full comprehension of the influence they mutually possess over each other.

¶ ix. *Disruptive discharge and insulation.*

1359. The next form of discharge has been distinguished by the adjective *disruptive* (1319.), as it in every case displaces more or less the particles amongst and across which it suddenly breaks. I include under it, discharge in the form of sparks, brushes, and glow (1405.), but exclude the cases of currents of air, fluids, &c., which, though frequently accompanying the former, are essentially distinct in their nature.

1360. The conditions requisite for the production of an electric spark in its simplest form are well known. An insulating dielectric must be interposed between two conducting surfaces in opposite states of electricity, and then if the actions be continually increased in strength, or otherwise favoured, either by exalting the electric state of the two conductors, or bringing them nearer to each other, or diminishing the density of the dielectric, a *spark* at last appears, and the two forces are for the time annihilated, for *discharge* has occurred.

1361. The conductors (which may be considered as the termini of the inductive action) are in ordinary cases most generally metals, whilst the dielectrics usually employed are common air and glass. In my view of induction, however, every dielectric becomes of importance, for as the results are considered essentially dependent on these bodies, it was to be expected that differences of action never before suspected would be evident upon close examination, and so at once give fresh confirmation of the theory, and open new doors of discovery into the extensive and varied fields of our science. This hope was especially entertained with respect to the gases, because of their high degree of insulation, their uniformity in physical condition, and great difference in chemical properties.

1362. All the effects prior to the discharge are inductive; and the degree of tension which it is necessary to attain before the spark passes is therefore, in the examination I am now making of the new view of induction, a very important point. It is the

* Philosophical Transactions, 1827, p. 22.

limit of the influence which the dielectric exerts in resisting discharge; it is a measure, consequently, of the conservative power of the dielectric, which in its turn may be considered as becoming a measure, and therefore a representative of the intensity of the electric forces in activity.

1363. Many philosophers have examined the circumstances of this limiting action in air, but, as far as I know, none have come near Mr. HARRIS as to the accuracy with, and the extent to, which he has carried on his investigations*. Some of his results I must very briefly notice, premising that they are all obtained with the use of air as the *dielectric* between the conducting surfaces.

1364. First as to the *distance* between the two balls used, or in other words, the *thickness* of the dielectric across which the induction was sustained. The quantity of electricity, measured by a unit jar or otherwise on the same principle with the unit jar, in the charged or inductive ball, necessary to produce spark discharge, was found to vary exactly with the distance between the balls, or between the discharging points, and that under very varied and exact forms of experiment†.

1365. Then with respect to variation in the *pressure or density* of the air. The quantities of electricity required to produce discharge across a *constant* interval varied exactly with variations of the density; the quantity of electricity and density of the air being in the same simple ratio. Or, if the quantity was retained the same, whilst the interval and density of the air were varied, then these were found in the inverse simple ratio of each other, the same quantity passing across twice the distance with air rarefied to one half‡.

1366. It must be remembered that these effects take place without any variation of the inductive force by condensation or rarefaction of the air. That force remains the same in air§, and in all gases (1284. 1292.), whatever their rarefaction may be.

1367. Variation of the *temperature* of the air produced no variation of the quantity of electricity required to cause discharge across a given interval‖.

Such are the general results, which I have occasion for at present, obtained by Mr. HARRIS, and they appear to me to be unexceptionable.

1368. In the theory of induction founded upon a molecular action of the dielectric, we have to look to the state of that body principally for the cause and determination of the above effects. Whilst the induction continues, it is assumed that the particles of the dielectric are in a certain polarized state, the tension of this state rising higher in each particle as the induction is raised to a higher degree, either by approximation of the inducing surfaces, variations of form, increase of the original force, or other means; until at last, the tension of the particles having reached the utmost degree which they can sustain without subversion of the whole arrangement, discharge immediately after takes place.

1369. The theory does not assume, however, that *all* the particles of the dielectric

* Philosophical Transactions, 1834, p. 225. † Ibid. ‡ Ibid. p. 229.
§ Ibid. pp. 237. 244. ‖ Ibid. p. 230.

subject to the inductive action are affected to the same amount, or acquire the same tension. What has been called the lateral action of the lines of inductive force (1231. 1297.), and the diverging and occasionally curved form of these lines, is against such a notion. The idea is, that any section taken through the dielectric across the lines of inductive force, and including *all of them*, would be equal, in the sum of the forces, to the sum of the forces in any other section; and that, therefore, the whole amount of tension for each such section would be the same.

1370. Discharge probably occurs, not when all the particles have attained to a certain degree of tension, but when that particle which is most affected has been exalted to the subverting or turning point (1410.). For though *all* the particles in the line of induction resist charge, and are associated in their actions so as to give a sum of resisting force, yet when any one is brought up to the overturning point, *all* must give way in the case of a spark between ball and ball. The breaking down of that one must of necessity cause the whole barrier to be overturned, for it was at its utmost degree of resistance when it possessed the aiding power of that one particle, in addition to the power of the rest, and the power of that one is now lost. Hence *tension* or *intensity** may, according to the theory, be considered as represented by the particular condition of the particles, or the amount in them of forced variation from their normal state (1298. 1368.).

1371. The whole effect produced by a charged conductor on a distant conductor, insulated or not, is by my theory assumed to be due to an action propagated from particle to particle of the intervening and insulating dielectric, the particles being considered as thrown for the time into a forced condition, from which they endeavour to return to their normal or natural state. The theory, therefore, seems to supply an easy explanation of the influence of *distance* in affecting induction (1303. 1364.). As the distance is diminished induction increases; for there are then fewer particles in the line of inductive force to oppose their resistance to the assumption of the forced or polarized state, and *vice versâ*. Again, as the distance diminishes, discharge across happens with a lower charge of electricity; for if, as in Harris's experiments (1364.), the interval be diminished to one half, then half the electricity required to discharge across the first interval is sufficient to strike across the second; and it is evident, also, that at that time there are only half the number of interposed molecules uniting their forces to resist the discharge.

1372. The effect of enlarging the conducting surfaces which are opposed to each other in the act of induction, is, if the electricity be limited in its supply, to lower the intensity of action; and this follows as a very natural consequence from the increased area of the dielectric across which the induction is effected. For by diffusing the inductive action, which at first was exerted through one square inch of sectional area of the dielectric, over two or three square inches of such area, twice or three times the number of molecules of the dielectric are brought into the polarized con-

* See Harris on proposed particular meaning of these terms, Philosophical Transactions, 1834, p. 222.

dition, and employed in sustaining the inductive action, and consequently the tension belonging to the smaller number on which the limited force was originally accumulated, must fall in a proportionate degree.

1373. For the same reason diminishing these opposing surfaces must increase the intensity up to the condition even of their becoming points. But in this case, the tension of the particles of the dielectric next the points is higher than that of particles midway, because of the lateral action and consequent bulging, as it were, of the lines of inductive force at the middle distance (1369.).

1374. The more exalted effects of induction on a point p, or any small surface, as the rounded end of a rod, opposed to a large surface, as that of a ball or plate, than when it is opposed to another point or end at the same distance, falls into harmonious relation (1302.). For in the latter case, the small surface p is affected only by those particles which are brought into the inductive condition by the equally small surface of the opposed conductor, whereas when that is a ball or plate the lines of inductive force from the latter are concentrated, as it were, upon the end p. Now though the molecules of the dielectric against the large surface may have a much lower state of tension than those against the similar smaller surface, yet they are also far more numerous, and, as the lines of inductive force converge towards a point, are able to communicate to the particles contained in any cross section (1369.) nearer the small surface an amount of tension equal to their own, and consequently much higher for each individual particle; so that, at the surface of the smaller conductor, the tension of a particle rises much, and if that conductor were to terminate in a point, the tension would rise to an infinite degree, except that it is limited, as before (1368.), by discharge. The nature of the discharge from small surfaces and points under induction will be resumed hereafter (1425. &c.).

1375. *Rarefaction* of the air does not alter the *intensity* of inductive action (1284. 1287.); nor is there any reason, as far as I can perceive, why it should. If the quantity of electricity and the distance remain the same, and the air be rarefied one half, then, though one half of the particles of the dielectric are removed, the other half assume a double degree of tension in their polarity, and therefore the inductive forces are balanced, and the result remains unaltered as long as the induction and insulation are sustained. But the case of *discharge* is very different; for as there are only half the number of dielectric particles in the rarefied atmosphere, so these are brought up to the discharging intensity by half the former quantity of electricity; discharge, therefore, ensues, and such a consequence of the theory is in perfect accordance with Mr. Harris's results (1365.).

1376. The *increase* of electricity required to cause discharge over the same distance, when the pressure of the air or its density is increased, flows in a similar manner, and on the same principle, from the molecular theory.

1377. Here I think my view of induction has a decided advantage over others, especially over that which refers the retention of electricity on the surface of conductors

in air to the *pressure of the atmosphere*. The latter is the view which, being adopted by Poisson and Biot*, is also, I believe, that generally received; and it relates two such dissimilar things, as the ponderous air and the subtil and even hypothetical fluid or fluids of electricity, by gross mechanical relations; by the bonds of mere static pressure. My theory, on the contrary, sets out at once by connecting the electric forces with the particles of matter; it derives all its proofs, and even its origin in the first instance, from experiment; and then, without any further assumption, seems to offer at once a full explanation of these and many other singular, peculiar, and, I think, heretofore unrelated effects.

1378. An important assisting experimental argument may here be adduced, derived from the difference of specific inductive capacity of different dielectrics (1269. 1274. 1278.). Consider an insulated sphere electrified positively and placed in the centre of another and larger sphere uninsulated, a uniform dielectric, as air, intervening. The case is really that of my apparatus (1187.), and also, in effect, that of any ball electrified in a room and removed to some distance from irregularly formed conductors. Whilst things remain in this state the electricity is distributed (so to speak) uniformly over the surface of the electrified sphere. But introduce such a dielectric as sulphur or lac, into the space between the two conductors on one side only, or opposite one part of the inner sphere, and immediately the electricity on the latter is diffused unequally (1229. 1270. 1309.), although the form of the conducting surfaces, their distances, and the *pressure* of the atmosphere remain perfectly unchanged.

1379. Fusinieri took a different view from that of Poisson, Biot, and others, of the reason why rarefaction of air caused easy diffusion of electricity. He considered the effect as due to the removal of the *obstacle* which the air presented to the expansion of the substances from which the electricity passed†. But platina balls show the phenomena in vacuo as well as volatile metals and other substances; besides which, when the rarefaction is very considerable, the electricity passes with scarcely any resistance, and the production of no sensible heat; so that I think Fusinieri's view of the matter is likely to gain but few assents.

1380. I have no need to remark upon the discharging or collecting power of flame or hot air. I believe, with Harris, that the mere heat does nothing (1367.), the rarefaction only being influential. The effect of rarefaction has been already considered generally (1375.); and that caused by the heat of a burning light, with the pointed form of the wick, and the carrying power of the carbonaceous particles which for the time are associated with it, are fully sufficient to account for all the effects.

1381. We have now arrived at the important question, how will the inductive tension requisite for insulation and disruptive discharge be sustained in gases, which,

* Ency. Britann. Supplement, vol. iv. Article Electricity, pp. 76. 81. &c.

† Bib. Univ. 1831. xlviii. 375.

having the same physical state and also the *same pressure* and the *same temperature* as *air*, differ from it in specific gravity, in chemical qualities, and it may be in peculiar relations, which not being as yet recognised, are purely electrical (1361.)?

1382. Into this question I can enter now only as far as is essential for the present argument, namely, that insulation and inductive tension do not depend merely upon the charged conductors employed, but also, and essentially, upon the interposed dielectric, in consequence of the molecular action of its particles.

1383. A glass vessel *a* (fig. 13.)* was ground at the top and bottom so as to be closed by two ground brass plates, *b* and *c*; *b* carried a stuffing box, with a sliding rod *d* terminated by a brass ball *s* below, and a ring above. The lower plate was connected with a foot, stop-cock, and socket, *e*, *f* and *g*; and also with a brass ball *l*, which by means of a stem attached to it and entering the socket *g*, could be fixed at various heights. The metallic parts of this apparatus were not varnished, but the glass was well covered with a coat of shell-lac previously dissolved in alcohol. On exhausting the vessel at the air-pump it could be filled with any other gas than air, and, in such cases, the gas so passed in was dried whilst entering by fuzed chloride of calcium.

1384. The other part of the apparatus consisted of two insulating pillars, *h* and *i*, to which were fixed two brass balls, and through these passed two sliding rods, *k* and *m*, terminated at each end by brass balls; *n* is the end of an insulated conductor, which could be rendered either positive or negative from an electrical machine; *o* and *p* are wires connecting it with the two parts previously described, and *q* is a wire which, connecting the two opposite sides of the collateral arrangements, also communicates with a good discharging train *r* (292.).

1385. It is evident that the discharge from the machine electricity may pass either between *s* and *l*, or S and L. The regulation adopted in the first experiments was to keep *s* and *l* with their distance *unchanged*, but to introduce first one gas and then another into the vessel *a*, and then balance the discharge at the one place against that at the other; for by making the interval at *u* sufficiently small, all the discharge would pass there, or making it sufficiently large it would all occur at the interval *v* in the receiver. On principle it seemed evident, that in this way the varying interval *u* might be taken as a measure, or rather indication of the resistance to discharge through the gas at the constant interval *v*. The following are the constant dimensions.

Ball *s*	0·93 of an inch.
Ball S	0·96 of an inch.
Ball *l*	2·02 of an inch.
Ball L	1·95 of an inch.
Interval *v*	0·62 of an inch.

1386. On proceeding to experiment it was found that when air or any gas was in

* The drawing is to a scale of $\frac{1}{2}$.

the receiver *a*, the interval *u* was not a fixed one; it might be altered through a certain range of distance, and yet sparks pass either there or at *v* in the receiver. The extremes were therefore noted, i. e. the greatest distance short of that at which the discharge *always* took place at *v* in the gas, and the least distance short of that at which it *always* took place at *u* in the air. Thus, with air in the receiver, the extremes at *u* were 0·56 and 0·79 of an inch, the range of 0·23 being one at which sparks passed occasionally either at one interval or the other.

1387. The small balls *s* and S could be rendered either positive or negative from the machine, and as gases were expected and were found to differ from each other in relation to this change, the results obtained under these differences of charge were also noted.

1388. The following is a Table of results; the gas named is that in the vessel *a*. The smallest, greatest, and mean interval at *u* in air is expressed in parts of an inch, the interval *v* being constantly 0·62 of an inch.

	Smallest.	Greatest.	Mean.
Air, *s* and S, pos.	0·60	0·79	0·695
Air, *s* and S, neg.	0·59	0·68	0·635
Oxygen, *s* and S, pos.	0·41	0·60	0·505
Oxygen, *s* and S, neg.	0·50	0·52	0·510
Nitrogen, *s* and S, pos.	0·55	0·68	0·615
Nitrogen, *s* and S, neg.	0·59	0·70	0·645
Hydrogen, *s* and S, pos.	0·30	0·44	0·370
Hydrogen, *s* and S, neg.	0·25	0·30	0·275
Carbonic acid, *s* and S, pos.	0·56	0·72	0·640
Carbonic acid, *s* and S, neg.	0·58	0·60	0·590
Olefiant gas, *s* and S, pos.	0·64	0·86	0·750
Olefiant gas, *s* and S, neg.	0·69	0·77	0·730
Coal gas, *s* and S, pos.	0·37	0·61	0·490
Coal gas, *s* and S, neg.	0·47	0·58	0·525
Muriatic acid gas, *s* and S, pos.	0·89	1·32	1·105
Muriatic acid gas, *s* and S, neg.	0·67	0·75	0·720

1389. The above results were all obtained at one time. On other occasions other experiments were made, which gave generally the same results as to order, though not as to numbers. Thus:

Hydrogen, *s* and S, pos.	0·23	0·57	0·400
Carbonic acid, *s* and S, pos.	0·51	1·05	0·780
Olefiant gas, *s* and S, pos.	0·66	1·27	0·965

I did not notice the difference of the barometer on the days of experiment.

1390. One would have expected only two distances, one for each interval, for which

the discharge might happen either at one or the other; and that the least alteration of either would immediately cause one to predominate constantly over the other. But that under common circumstances is not the case. With air in the receiver, the variation amounted to 0·2 of an inch nearly on the smaller interval of 0·6, and with muriatic acid gas, the variation was above 0·4 on the smaller interval of 0·9. Why is it that when a fixed interval (the one in the receiver) will pass a spark that cannot go across 0·6 of air at one time, it will immediately after, and apparently under exactly similar circumstances, not pass a spark that can go across 0·8 of air?

1391. It is probable that part of this variation will be traced to particles of dust in the air drawn into and about the circuit. I believe also that part depends upon a variable charged condition of the surface of the glass vessel *a*. That the whole of the effect is not traceable to the influence of circumstances in the vessel *a*, may be deduced from the fact, that when sparks occur between balls in free air they frequently are not straight, and often pass otherwise than by the shortest distance. These variations in air itself, and at different parts of the very same balls, show the presence and influence of circumstances which are calculated to produce effects of the kind now under consideration.

1392. When a spark had passed at either interval, then, generally, more tended to appear at the *same* interval, as if a preparation had been made for the passing of the latter sparks. So also on continuing to work the machine quickly the sparks generally followed at the same place. This effect is probably due in part to the warmth of the air heated by the preceding spark, in part to dust, and I suspect in part to something unperceived as yet in the circumstances of discharge.

1393. A very remarkable difference, which is *constant* in its direction, occurs when the electricity communicated to the balls *s* and S is changed from positive to negative, or in the contrary direction. It is that the range of variation is always greater when the small balls are positive than when they are negative. This is exhibited in the following Table, drawn from the former experiments.

	Pos.	Neg.
In Air the range was . .	0·19	0·09
Oxygen	0·19	0·02
Nitrogen	0·13	0·11
Hydrogen	0·14	0·05
Carbonic acid	0·16	0·02
Olefiant gas	0·22	0·08
Coal gas	0·24	0·12
Muriatic acid	0·43	0·08

I have no doubt these numbers require considerable correction, but the general result is striking, and the differences in several cases very great.

1394. Though, in consequence of the variation of the striking distance (1386.), the interval in air fails to be a measure, as yet, of the insulating or resisting power of the

gas in the vessel, yet we may for present purposes take the mean interval as representing in some degree that power. On examining these mean intervals as they are given in the third column (1388.), it will be very evident, that gases, when employed as dielectrics, have peculiar electrical relations to insulation, and therefore to induction, very distinct from such as might be supposed to depend upon their mere physical qualities of specific gravity or pressure.

1395. First, it is clear that at the *same pressure* they are not alike, the difference being as great as 37 and 110. When the small balls are charged positively, and with the same surfaces and the same pressure, muriatic acid gas has three times the insulating or restraining power (1362.) of hydrogen gas, and nearly twice that of oxygen, nitrogen, or air.

1396. Yet it is evident that the difference is not due to specific gravity, for though hydrogen is the lowest, and therefore lower than oxygen, oxygen is much beneath nitrogen, or than olefiant gas; and carbonic acid gas, though considerably heavier than olefiant gas or muriatic gas, is lower than either. Oxygen as a heavy, and olefiant as a light gas, are in strong contrast with each other; and if we may reason of olefiant gas from HARRIS's results with air (1365.), then it might be rarefied to two-thirds its usual density, or to a specific gravity of 9·3 (hydrogen being 1), and having neither the same density nor pressure as oxygen, would have equal insulating powers with it, or equal tendency to resist discharge.

1397. Experiments have already been described (1291. 1292.) which show that the gases are sensibly alike in their inductive capacity. This result is not in contradiction with the existence of great differences in their restraining power. The same point has been observed already in regard to dense and rare air (1375.).

1398. Hence arises a new argument proving that it cannot be mere pressure of the atmosphere which prevents or governs discharge (1377. 1378.), but a specific electric quality or relation of the gaseous medium. Hence also additional argument for the theory of molecular inductive action.

1399. Other specific differences amongst the gases may be drawn from the preceding series of experiments, rough and hasty as they are. Thus the positive and negative series of mean intervals do not give the same differences. It has been already noticed that the negative numbers are lower than the positive (1393.), but, besides that, the *order* of the positive and negative results is not the same. Thus on comparing the mean numbers (which represent for the present insulating tension,) it appears that in air, hydrogen, carbonic acid, olefiant gas and muriatic acid, the tension rose higher when the smaller ball was made positive than when rendered negative, whilst in oxygen, nitrogen, and coal gas the reverse was the case. Now though the numbers cannot be trusted as exact, and though air, oxygen, and nitrogen should probably be on the same side, yet some of the results, as, for instance, those with muriatic acid, fully show a peculiar relation and difference amongst gases in this respect. This was further proved by making the interval in air 0·8 of an inch whilst

muriatic acid gas was in the vessel *a*; for on charging the small balls *s* and S positively, *all* the discharge took place through the *air*; but on charging them negatively, *all* the discharge took place through the *muriatic acid gas*.

1400. So also, when the conductor *n* was connected *only* with the muriatic acid gas apparatus, it was found that the discharge was more facile when the small ball *s* was negative than when positive; for in the latter case, much of the electricity passed off as brush discharge through the air from the connecting wire *p*; but in the former case, it all seemed to go through the muriatic acid.

1401. The consideration, however, of positive and negative discharge across air and other gases will be resumed in the further part of this, or in the next paper.

1402. Here for the present I must leave this part of the subject, which had for its object only to observe how far gases agreed or differed as to their power of retaining a charge on bodies acting by induction through them. All the results conspire to show that Induction is an action of contiguous molecules (1295. &c.); but besides confirming this, the first principle placed for proof in the present inquiry, they greatly assist in developing the specific properties of each gaseous dielectric, at the same time showing that further and extensive experimental investigation is necessary, and holding out the promise of new discovery as the reward of the labour required.

1403. When we pass from the consideration of dielectrics like the gases to that of bodies having the liquid and solid condition, then our reasonings in the present state of the subject assume much more of the character of mere supposition. Still I do not perceive anything adverse to the theory in the phenomena which such bodies present. If we take three insulating dielectrics, as air, oil of turpentine and shell-lac, and use the same balls or conductors at the same intervals in these three substances, increasing the intensity of the induction until discharge take place, we shall find that it must be raised much higher in the fluid than for the gas, and higher still in the solid than for the fluid. Nor is this inconsistent with the theory; for with the liquid, though its molecules are free to move almost as easily as those of the gas, there are many more particles introduced into the given interval; and as respects the latter circumstance, the same is the case when the solid body is employed. Besides that, the cohesive force of the body used will produce some effect; for though the production of the polarized states in the particle of a solid may not be obstructed, but, on the contrary, may in some cases be even favoured (1164. 1344.) by its solidity or other circumstances, yet solidity may well exert an influence on the point of its final subversion, (just as it prevents discharge in an electrolyte,) and so enable inductive intensity to rise to a much higher degree.

1404. In the cases of solids and liquids too, bodies may, and most probably do, possess specific differences as to their ability of assuming the polarized state, and

also as to the extent to which that polarity must rise before discharge occurs. An analogous difference exists in the specific inductive capacities already pointed out in a few substances (1278.) in the last paper. Such a difference might even account for the various degrees of insulating and conducting power possessed by different bodies, and, if it should be found to exist, would add further strength to the argument in favour of the molecular theory of inductive action.

1405. Having considered these various cases of sustained insulation in non-conducting dielectrics up to the highest point which they can attain, we find that they terminate at last in *disruptive discharge*; the peculiar condition of the molecules of the dielectric which was necessary to the continuous induction, being equally essential to the occurrence of that effect which closes all the phenomena. This discharge is not only in its appearance and condition different to the former modes by which the lowering of the powers was effected (1320. 1343.), but, whilst really the same in principle, varies much from itself in certain characters, and thus presents us with the forms of *spark*, *brush*, and *glow* (1359.). I will first consider *the spark*, limiting it for the present to the case of discharge between two oppositely electrified conducting surfaces.

The electric spark or flash.

1406. The *spark* is a discharge or lowering of the polarized inductive state of many dielectric particles, by a particular action of a few of the particles occupying a very small and limited space; all the previously polarized particles returning to their first or normal condition in the inverse order in which they left it, and uniting their powers meanwhile to produce, or rather to continue, (1417 and 1436.) the discharge effect in the place where the subversion of force first occurred. My impression is, that the few particles situated where discharge occurs are not merely pushed apart, but assume a peculiar state, a highly exalted condition for the time, i. e. have thrown upon them all the surrounding forces in succession, and rising up to a proportionate intensity of condition, perhaps equal to that of chemically combining atoms, discharge the powers, possibly in the same manner as they do theirs, by some operation at present unknown to us; and so the end of the whole. The ultimate effect is exactly as if a metallic wire had been put into the place of the discharging particles; and it does not seem impossible that the principles of action in both cases may, hereafter, prove to be the same.

1407. The *path of the spark*, or of the discharge, depends on the degree of tension acquired by the particles in the line of discharge, circumstances, which in every common case are very evident and by the theory easy to understand, rendering it higher in them than in their neighbours, and, by exalting them first to the requisite condition, causing them to determine the course of the discharge. Hence the se-

lection of the path, and the solution of the wonder which HARRIS has so well described* as existing under the old theory. All is prepared amongst the molecules beforehand, by the prior induction, for the path either of the electric spark or of lightning itself.

1408. The same difficulty is expressed as a principle by NOBILI for voltaic electricity, almost in Mr. HARRIS's words, namely†, "electricity directs itself towards the point where it can most easily discharge itself," and the results of this as a principle he has well wrought out for the case of voltaic currents. But the *solution* of the difficulty, or the proximate cause of the effects, is the same: induction brings the particles up to or towards a certain state (1370.); and by those which first attain it, is the discharge first and most efficiently performed.

1409. The *moment* of discharge is probably determined by that molecule of the dielectric which, from the circumstances, has its tension most quickly raised up to the maximum intensity. In all cases where the discharge passes from conductor to conductor this molecule must be on the surface of one of them; but when it passes between a conductor and a non-conductor, it is, perhaps, not always so (1453.). When this particle has acquired its maximum tension, then the whole barrier of resistance is broken down in the line or lines of inductive action originating at it, and disruptive discharge occurs (1370.): and such an inference, drawn as it is from the theory, seems to me in accordance with Mr. HARRIS's facts and conclusions respecting the resistance of the atmosphere, namely, that it is not really greater at any one discharging distance than another‡.

1410. It seems probable, that the tension of a particle of the same dielectric, as air, which is requisite to produce discharge, is a *constant quantity*, whatever the shape of the part of the conductor with which it is in contact, whether ball or point; whatever the thickness or depth of dielectric throughout which induction is exerted; perhaps, even, whatever the state, as to rarefaction or condensation of the dielectric; and whatever the nature of the conductor, good or bad, with which the particle is for the moment associated. In saying so much, I do not mean to exclude small differences which may be caused by the reaction of neighbouring particles on the deciding particle, and indeed, it is evident that the intensity required in a particle must be related to the condition of those which are contiguous. But if the expectation should be found to approximate to truth, what a generality of character it presents! and, in the definiteness of the power possessed by a particular molecule, may we not hope to find an immediate relation to the force which, being electrical, is equally definite and constitutes chemical affinity?

1411. Theoretically it would seem that, at the moment of discharge by the spark in one line of inductive force, not merely would all the other lines throw their forces into this one (1406.), but the lateral effect, equivalent to a repulsion of these lines

* Nautical Magazine, 1834, p. 229.

† Bibliotheque Universelle, 1835, lix. 275.

‡ Philosophical Transactions, 1834, pp. 227, 229.

(1224. 1297.), would be relieved and, perhaps, followed by something equivalent to a contrary action, amounting to a collapse or attraction of these parts. Having long sought for some transverse force in statical electricity, which should be the equivalent to magnetism or the transverse force of current electricity, and conceiving that it might be connected with the transverse action of the lines of inductive force already described (1297.), I was desirous, by various experiments, of bringing out the effect of such a force, and making it bear upon the phenomena of electro-magnetism and magneto-electricity.

1412. Amongst other results, I expected and sought for the mutual affection, or even the lateral coalition of two similar sparks, if they could be obtained simultaneously side by side, and sufficiently near to each other. For this purpose, two similar Leyden jars were supplied with rods of copper projecting from their balls in a horizontal direction, the rods being about 0·2 of an inch thick, and rounded at the ends. The jars were placed upon a sheet of tinfoil, and so adjusted that their rods, *a* and *b*, were near together, in the position represented in plan at fig. 2. *c* and *d* were two brass balls connected by a brass rod and insulated: *e* was also a brass ball connected, by a wire, with the ground and with the tinfoil upon which the Leyden jars were placed. By laying an insulated metal rod across from *a* to *b*, charging the jars, and removing the rod, both the jars could be brought up to the same intensity of charge (1370.). Then, making the ball *e* approach the ball *d*, at the moment the spark passed there, two sparks passed between the rods *n*, *o*, and the ball *c*; and as far as the eye could judge, or the conditions determine, they were simultaneous.

1413. Under these circumstances two modes of discharge took place; either each end had its own particular spark to the ball, or else one end only was associated by a spark with the ball, but was at the same time related to the other end by a spark between the two.

1414. When the ball *c* was about an inch in diameter, the ends *n* and *o*, about half an inch from it, and about 0·4 of an inch from each other, the two sparks to the ball could be obtained. When, for the purpose of bringing the sparks nearer together, the ends, *n* and *o*, were brought closer to each other, then, unless very carefully adjusted, only one end had a spark with the ball, the other having a spark to it; and the least variation of position would cause either *n* or *o* to be the end which, giving the direct spark to the ball, was also the one through, or by means of which, the other discharged its electricity.

1415. On making the ball *c* smaller, I found that then it was needful to make the interval between the ends *n* and *o* larger in proportion to the distance between them and the ball *c*. On making *c* larger, I found I could diminish the interval, and so bring the two simultaneous separate sparks closer together, until, at last, the distance between them was not more at the widest part than 0·6 of their whole length.

1416. Numerous sparks were then passed and carefully observed. They were very rarely straight, but either curved or bent irregularly. In the average of cases they were, I think, decidedly convex towards each other; perhaps two thirds presented more or less of this effect, the rest bulging more or less outwards. I was never able, however, to obtain sparks which, separately leaving the ends of the wires *n* and *o*, conjoined into one spark before they reached or communicated with the ball *c*. At present, therefore, though I think I saw a tendency in the sparks to unite, I cannot assert it as a fact.

1417. But there is one very interesting effect here analogous to, and it may be in part the same with, that I was searching for: I mean the increased facility of discharge where the spark passes. For instance, in the cases where one end, as *n*, discharged the electricity of both ends to the ball *c*, fig. 2., the electricity of the other end *o*, had to pass through an interval of air 1·5 times as great as that which it might have taken, by its direct passage between the end and the ball itself. In such cases, the eye could not distinguish, even by the use of WHEATSTONE'S means*, that the spark from the end *n*, which contained both portions of electricity, was a double spark. It could not have consisted of two sparks taking separate courses, for such an effect would have been visible to the eye; but it is just possible, that the spark of the first end *n* and its jar, passing at the smallest interval of time before that of the other *o*, had heated and expanded the air in its course, and made it so much more favourable to discharge, that the electricity of the end *o* preferred leaping across to it and taking a very circuitous route, rather than the more direct one to the ball. It must, however, be remarked, in answer to this supposition, that the one spark between *d* and *e* would, by its influence, tend to produce simultaneous discharges at *n* and *o*, and certainly did so, when no preponderance was given to one wire over the other, as to the previous inductive effect (1414.).

1418. The fact, however, is, that disruptive discharge is favourable to itself. It is at the outset a case of tottering equilibrium: and if *time* be an element in discharge, in however minute a proportion (1436.), then the commencement of the act at any point favours its continuance and increase there, and portions of power will be discharged by a course which they would not otherwise have taken.

1419. The mere heating and expansion of the air itself by the first portion of electricity which passes, must have a great influence in producing this result.

1420. As to the result itself, we see its influence in every spark that passes; for it is not the whole quantity which passes that determines the discharge, but merely that small portion of force which brings the deciding molecule (1370.) up to its maximum tension; then, when its forces are subverted and discharge begins, all the rest passes by the same course, from the influence of the favouring circumstances just referred to; and whether it be the electricity on a square inch, or a thousand

* Philosophical Transactions, 1834, pp. 584, 585.

square inches of charged glass, the discharge is complete. Hereafter we shall find the influence of this effect in the formation of brushes (1435.); and it is not impossible that we may trace it producing the jagged spark and the forked lightning.

1421. The characters of the electric spark in *different gases* vary, and the variation *may* be due simply to the effect of the heat evolved at the moment. But it may also be due to that specific relation of the particles and the electric forces which I have assumed as the basis of a theory of induction; the facts do not oppose such a view; and in that view, the variation strengthens the argument for molecular action, as it would seem to show the influence of the latter in every part of the electrical effect (1423. 1454.).

1422. The appearances of the sparks in different gases have often been observed and recorded*, but I think it not out of place to notice briefly the following results; they were obtained with balls of brass, (platina surfaces would have been better,) and at common pressures. In *air*, the sparks have that intense light and bluish colour which are so well known, and often have faint or dark parts in their course, when the quantity of electricity passing is not great. In *nitrogen*, they are very beautiful, having the same general appearance as in air, but have decidedly more colour of a bluish or purple character, and I thought were remarkably sonorous. In *oxygen*, the sparks were whiter than in air or nitrogen, and I think not so brilliant. In *hydrogen*, they had a very fine crimson colour, not due to its rarity, for the character passed away as the atmosphere was rarefied (1459.)†. Very little sound was produced in this gas; but that is a consequence of its physical condition‡. In *carbonic acid gas*, the colour was similar to that of the spark in air, but with a little green in it: the sparks were remarkably irregular in form, more so than in common air: they could also, under similar circumstances as to size of ball, &c., be obtained much longer than in air, the gas showing a singular readiness to pass the discharge in the form of spark. In *muriatic acid gas*, the spark was nearly white: it was always bright throughout, never presenting those dark parts which happen in air, nitrogen, and some other gases. The gas was dry, and during the whole experiment the surface of the glass globe within remained quite dry and bright. In *coal gas*, the spark was sometimes green, sometimes red, and occasionally one part was green and another red. Black parts also occur very suddenly in the line of the spark, i. e. they are not connected by any dull part with bright portions, but the two seem to join directly one with the other.

1423. These varieties of character impress my mind with a feeling, that they are due to a direct relation of the electric powers to the particles of the dielectric through which the discharge occurs, and are not the mere results of a casual ignition or a

* See Van Marum's description of the Teylerian machine, vol. i. p. 112., and vol. ii. p. 196.; also Ency. Britan., vol. vi., Article Electricity, pp. 505, 507.

† Van Marum says they are about four times as large in hydrogen as in air, vol. i. p. 122.

‡ Leslie.

secondary kind of action of the electricity, upon the particles which it finds in its course and thrusts aside in its passage (1454.).

1424. The spark may be obtained in media which are far denser than air, as in oil of turpentine, olive oil, resin, glass, &c.: it may also be obtained in bodies which being denser likewise approximate to the condition of conductors, as spermaceti, water, &c. But in these cases, nothing occurs which, as far as I can perceive, is at all hostile to the general views I have endeavoured to advocate.

The electrical brush.

1425. The *brush* is the next form of disruptive discharge which I will consider. There are many ways of obtaining it, or rather of exalting its characters; and all these ways illustrate the principles upon which it is produced. If an insulated conductor, connected with the positive conductor of an electrical machine, have a metal rod 0·3 of an inch in diameter projecting from it outwards from the machine, and terminating by a rounded end or a small ball, it will generally give good brushes; or, if the machine be not in good action, then many ways of assisting the formation of the brush can be resorted to; thus, the hand or any *large* conducting surface may be approached towards the termination to increase inductive force (1374.): or the termination may be smaller and of badly conducting matter, as wood: or sparks may be taken between the prime conductor of the machine and the secondary conductor to which the termination giving brushes belongs: or, which gives to the brushes exceedingly fine characters and great magnitude, the air around the termination may be rarefied more or less, either by heat or the air pump; the former favourable circumstances being also continued.

1426. The brush when obtained by a powerful machine on a ball about 0·7 of an inch in diameter, at the end of a long brass rod attached to the positive prime conductor, had the general appearance as to form represented in fig. 3.: a short conical bright part or root appeared at the middle part of the ball projecting directly from it, which, at a little distance from the ball, broke out suddenly into a wide brush of pale ramifications having a quivering motion, and being accompanied at the same time with a low dull chattering sound.

1427. At first the brush seems continuous, but Professor WHEATSTONE has shown that the whole phenomenon consists of successive intermitting discharges*. If the eye be passed rapidly, not by a motion of the head, but of the eyeball itself, across the direction of the brush, by first looking steadfastly about 10° or 15° above, and then instantly as much below it, the general brush will be resolved into a number of individual brushes, standing in a row upon the line which the eye passed over; each elementary brush being the result of a single discharge, and the space between them representing both the time during which the eye was passing over that space, and that which elapsed between one discharge and another.

* Philosophical Transactions, 1834, p. 586.

1428. The single brushes could easily be separated to eight or ten times their own width, but were not at the same time extended, i. e. they did not become more indefinite in shape, but, on the contrary, less so, each being more distinct in form, ramification, and character, because of its separation from the others, in its effects upon the eye. Each, therefore, was instantaneous in its existence (1436.). Each had the conical root complete (1426.).

1429. On using a smaller ball, the general brush was smaller, and the sound, though weaker, more continuous. On resolving the brush into its elementary parts, as before, these were found to occur at much shorter intervals than in the former case, but still the discharge was intermitting.

1430. Employing a wire with a round end, the brush was still smaller, but, as before, separable into successive discharges. The sound, though feebler, was higher in pitch, being a distinct musical note.

1431. The sound is, in fact, due to the recurrence of the noise of each separate discharge, and these, happening at intervals nearly equal under ordinary circumstances, cause a definite note to be heard, which, rising in pitch with the increased rapidity and regularity of the intermitting discharges, gives a ready and accurate measure of the intervals, and so may be used in any case when the discharge is heard, even though the appearances may not be seen, to determine the element of *time*. So also, when, by bringing the hand towards a projecting rod or ball, the pitch of the tone produced by a brushy discharge increases, the effect informs us that we have increased the induction (1374.), and by that means increased the rapidity of the alternations of charge and discharge.

1432. By using wires with finer terminations, smaller brushes were obtained, until they could hardly be distinguished as brushes; but as long as *sound* was heard, the discharge could be ascertained by the eye to be intermitting; and when the sound ceased, the light became *continuous* as a glow (1359. 1405.).

1433. To those not accustomed to use the eye in the manner I have described, or, in cases where the recurrence is too quick for any unassisted eye, the beautiful revolving mirror of Professor WHEATSTONE* will be useful for such developments of condition as those mentioned above. Another excellent process is to produce the brush or other luminous phenomenon on the end of a rod held in the hand opposite to a charged positive or negative conductor, and then move the rod rapidly from side to side whilst the eye remains still. The successive discharges occur of course in different places, and the state of things before, at, and after a single coruscation or brush can be exceedingly well separated.

1434. The *brush* is in reality a discharge between a bad or a non-conductor and either a conductor or another non-conductor. Under common circumstances, the brush is a discharge between a conductor and air, and I conceive it to take place in something like the following manner. When the end of an electrified rod projects into

* Philosophical Transactions, 1834, pp. 584, 585.

the middle of a room, induction takes place between it and the walls of the room, across the dielectric, air; and the lines of inductive force accumulate upon the end in greater quantity than elsewhere, or the particles of air at the end of the rod are more highly polarized than those at any other part of the rod, for the reasons already given (1374.). The particles of air situated in sections across these lines of force are least polarized in sections towards the walls, and most polarized in those nearer to the end of the wires (1369.): thus, it may well happen, that a particle at the end of the wire is at a tension that will immediately terminate in discharge, whilst in those even only a few inches off, the tension is still beneath that point. But suppose the rod to be charged positively, a particle of air A, fig. 4. next it, being polarized, and having of course its negative force directed towards the rod and its positive force outwards; the instant that discharge takes place between the positive force of the particle of the rod opposite the air and the negative force of the particle of air towards the rod, the whole particle of air becomes positively electrified; and when, the next instant, the discharged part of the rod resumes its positive state, by conduction from the surface of metal behind, it not only acts on the particles beyond A, by throwing A into a polarized state again, but A itself, because of its charged state, exerts a distinct inductive act towards these further particles, and the tension is consequently so much exalted between A and B, that discharge takes place there also, as well as again between the metal and A.

1435. In addition to this effect, it has been shown, that, the act of discharge having once commenced, the whole operation, like a case of unstable equilibrium, is hastened to a conclusion (1370. 1418.), the rest of the act being facilitated in its occurrence, and other electricity than that which caused the first necessary tension hurrying to the spot. When, therefore, disruptive discharge has once commenced at the root of a brush, the electric force which has been accumulating in the conductor attached to the rod, finds a more ready discharge there than elsewhere, and will at once follow the course marked out as it were for it, thus leaving the conductor in a partially discharged state, and the air about the end of the wire in a charged condition; and the time necessary for restoring the full charge of the conductor, and the dispersion of the charged air in a greater or smaller degree, by the joint forces of repulsion from the conductor and attraction towards the walls of the room, to which its inductive action is directed, is just that time which forms the interval between brush and brush (1420. 1427. 1431.).

1436. The words of this description are long, but there is nothing in the act or the forces on which it depends to prevent its being *instantaneous*, as far as we can estimate and measure it. The consideration of *time* is, however, important in several points of view (1418.), and in reference to disruptive discharge, it seemed from theory far more probable that it might be detected in a brush than in a spark, for in a brush, the particles in the line through which the discharge passes are in very different states as to intensity, and the discharge is already complete in its act at the root of

the brush, before the particles at the extremity of the ramifications have yet attained their maximum intensity.

1437. I consider brush discharge as, probably, a successive effect in this way. Discharge begins at the root (1426.), and, extending itself in succession to all parts of the single brush, continues to go on at the root and the previously formed parts until the whole brush is complete; then, by the fall in intensity and power at the conductor, it ceases at once in all parts, to be renewed, when that power has risen again to a sufficient degree. But in a spark, the particles in the line of discharge being, from the circumstances, nearly alike in their intensity of polarization, suffer discharge so nearly at the same moment as to make the time quite insensible to us.

1438. Mr. WHEATSTONE has already made experiments which fully illustrate this point. He found that the brush generally had a sensible duration, but that with his highest capabilities he could not detect any such effect in the spark*. I repeated his experiment on the brush, though with more imperfect means, to ascertain whether I could distinguish a longer duration in the stem or root of the brush than in the extremities, and the appearances were such as to make me think an effect of this kind was produced.

1439. That the discharge breaks into several ramifications, and by them passes through portions of air alike, or nearly alike, as to polarization and the degree of tension the particles there have acquired, is a very natural result of the previous state of things, and sooner to be expected than that the discharge should continue to go straight out into space in a single line amongst those particles which, being at a distance from the end of the rod, are in a lower state of tension than those which are near: and whilst we cannot but conclude, that those parts where the branches of a single brush appear, are more favourably circumstanced for discharge than the darker parts between the ramifications, we may also conclude, that in those parts where the light of concomitant discharge is equal, there the circumstances are nearly equal also. The single brushes are by no means of the same particular shape even when they are observed without displacement of the rod or surrounding objects (1427. 1433.), and the successive discharges may be considered as taking place into the mass of air around, through different roads at each brush, according as minute circumstances, as dust, &c. (1391. 1392.) may have favoured the course by one set of particles rather than another.

1440. Brush discharge does not essentially require any current of the medium in which the brush appears: the current almost always occurs, but is a consequence of the brush, and will be considered hereafter. On holding a blunt point positively charged towards uninsulated water, a star or glow appeared on the point, a current of air passed from it, and the surface of the water was depressed; but on bringing

* Philosophical Transactions, 1836, pp. 586, 590.

the point so near that sonorous brushes passed, then the current of air instantly ceased, and the surface of the water became level.

1441. The discharge by a brush is not to all the particles of air that are near the electrified conductor from which the brush issues; only those parts where the ramifications pass are electrified: the air in the central dark parts between them receives no charge, and, in fact, at the time of discharge, has its electric and inductive tension considerably lowered. For consider fig. 14. to represent a single positive brush;—the induction before the discharge is from the end of the rod outwards, in diverging lines towards the distant conductors, as the walls of the room, &c., and a particle at *a* has polarity of a certain degree of tension, and tends with a certain force to become charged; but at the moment of discharge, the air in the ramifications *b* and *d*, acquiring also a positive state, opposes its influence to that of the positive conductor on *a*, and the tension of the particle at *a* is therefore diminished rather than increased. The charged particles at *b* and *d* are now inductive bodies, but their lines of inductive action are still outwards towards the walls of the room; the direction of the polarity and the tendency of other particles to charge from these, being governed by, or in conformity with, these lines of force.

1442. The particles that are charged are probably very highly charged, but, the medium being a non-conductor, they cannot communicate that state to their neighbours. They travel, therefore, under the influence of the repulsive and attractive forces, from the charged conductor towards the nearest uninsulated conductor, or the nearest body in a different state to themselves, just as charged particles of dust would travel, and are then discharged; each particle acting, in its course, as a centre of inductive force upon any bodies near which it may come.

1443. The travelling of these charged particles when they are numerous, causes wind and currents, but these will come into consideration under *carrying discharge* (1319.). When air is said to be electrified, and it frequently assumes this state near electrical machines, it consists, according to my view, of a mixture of electrified and unelectrified particles, the latter being in very large proportion to the former. When we gather electricity from air by a flame or by wires, it is either by the actual discharge of these particles, or by effects dependent on their inductive action, a case of either kind being produceable at pleasure. That the law of equality between the two forces or forms of force in inductive action is as strictly preserved in these as in other cases, is fully shown by the fact, formerly stated (1173. 1174.), that, however strongly air in a vessel might be charged positively, there was an exactly equal amount of negative force on the inner surface of the vessel itself, for no residual portion of either the one or the other electricity could be obtained.

1444. I have nowhere said, nor does it follow, that the air is charged only where the luminous brush appears. The charging may extend beyond those parts which are visible, i. e. particles to the right or left of the lines of light may receive electri-

city, the parts which are luminous being so only because much electricity is passing by them to other parts (1437.); just as in a spark discharge the light is greater as more electricity passes, though it has no necessary relation to the quantity required to commence discharge (1370. 1420.). Hence the form we see in a brush may by no means represent the whole quantity of air electrified; for an invisible portion, clothing the visible form to a certain depth, may, at the same time, receive its charge.

1445. Several effects which I have met with in muriatic acid gas tend to make me believe, that that gaseous body allows of a dark discharge. At the same time, it is quite clear from theory, that in some gases, the reverse of this may occur, i. e. that the charging of the air may not extend even so far as the light. We do not know as yet enough of the electric light to be able to state on what it depends, and it is very possible that, when electricity bursts forth into air, all the particles of which are in a state of tension, light may be evolved by such as, being very near to, are not of, those which actually receive a charge at the time.

1446. The further a brush extends in a gas, the further no doubt is the charge or discharge carried forward; but this may vary between different gases, and yet the intensity required for the first moment of discharge not vary in the same, but in some other proportion. Thus with respect to nitrogen and muriatic acid gases, the former, as far as my experiments have proceeded, produces far finer and larger brushes than the latter (1458. 1462.), but the intensity required to commence discharge is much higher for the latter than the former (1395.). Here again, therefore, as in many other qualities, specific differences are presented by different gaseous dielectrics, and so prove the special relation of the latter to the act and the phenomena of induction.

1447. To sum up these considerations respecting the character and condition of the brush, I may state that it is a spark to air; a diffusion of electric force to matter, not by conduction, but disruptive discharge; a dilute spark which, passing to very badly conducting matter, frequently discharges but a small portion of the power stored up in the conductor; for as the air charged reacts on the conductor, whilst the conductor, by loss of electricity, sinks in its force, the discharge quickly ceases, until by the dispersion of the charged air and the renewal of the excited conditions of the conductor, circumstances have risen up to their first effective condition, again to cause discharge, and again to fall and rise.

1448. The brush and spark gradually pass into one another. Making a small ball positive by a good electrical machine with a large prime conductor, and approaching a large uninsulated discharging ball towards it, very beautiful variations from the spark to the brush may be obtained. The drawings of long and powerful sparks, given by Van Marum*, Harris†, and others, also indicate the same phenomena. As far as I have observed, whenever the spark has been brushy in air of common press-

* Description of the Teylerian machine, vol. i. pp. 28. 32.; vol. ii. p. 226, &c.

† Philosophical Transactions, 1834, p. 243.

ures, the whole of the electricity has not been discharged, but only portions of it, more or less according to circumstances: whereas, whenever the effect has been a distinct spark throughout the whole of its course, the discharge has been perfect, provided no interruption had been made to it elsewhere, in the discharging circuit, than where the spark occurred.

1449. When an electrical brush from an inch to six inches in length or more is issuing into free air, it has the form given, fig. 3. But if the hand, a ball, or any knobbed conductor be brought near, the extremities of the coruscations turn towards it and each other, and the whole assumes various forms according to circumstances, as in figs. 5, 6, and 7. The influence of the circumstances in each case is easily traced, and I might describe it here, but that I should be ashamed to occupy the time of the Society in things so evident. But how beautifully does the curvature of the ramifications illustrate the curved form of the lines of inductive force existing previous to the discharge! for the former are consequences of the latter, and take their course, in each discharge, where the previous inductive tension had been raised to the proper degree. They represent these curves just as well as iron filings represent magnetic curves, the visible effects in both cases being the consequences of the action of the forces in *the places where* the effects appear. The phenomena, therefore, constitute additional and powerful testimony (1216. 1230.) to that already given in favour both of induction through dielectrics in curved lines (1231.), and of the lateral relation of these lines, by an effect equivalent to a repulsion producing divergence, or, as in the cases figured, the bulging form.

1450. In reference to the theory of molecular inductive action, I may also add here, the proof deducible from the long brushy ramifying spark which may be obtained between a small ball on the positive conductor of an electrical machine, and a larger one at a distance (1448.). What a fine illustration that spark affords of the previous condition of *all* the particles of the dielectric between the surfaces of discharge, and how unlike the appearances are to any which would be deduced from the theory which assumes inductive action to be action at a distance, in straight lines only; and charge, as being electricity retained upon the surface of conductors by the mere pressure of the atmosphere!

1451. When the brush is obtained in rarefied air, the appearances vary greatly, according to circumstances, and are exceedingly beautiful. Sometimes a brush may be formed of only six or seven branches, these being broad and highly luminous, of a purple colour, and in some parts an inch or more apart:—by a spark discharge at the prime conductor (1455.) single brushes may be obtained at pleasure. Discharge in the form of a brush is favoured by rarefaction of the air, in the same manner and for the same reason as discharge in the form of a spark (1375.); but in every case

there is previous induction and charge through the dielectric, and polarity of its particles (1437.), the induction being, as in any other instance, alternately raised by the machine and lowered by the discharge. In certain experiments the rarefaction was increased to the utmost degree, and the opposed conducting surfaces brought as near together as possible without producing the glow: the brushes then contracted in their lateral dimensions, and recurred so rapidly as to form an apparently continuous arc of light from metal to metal. Still the discharge could be observed to intermit (1427.), so that even under these high conditions, induction preceded each single brush, and the tense polarized condition of the contiguous particles was a necessary preparation for the discharge itself.

1452. The brush form of disruptive discharge may be obtained not only in air and gases, but also in much denser media. I procured it in oil of turpentine from the end of a wire going through a glass tube into the fluid contained in a metal vessel. The brush was small and very difficult to obtain; the ramifications were simple, and stretched out from each other diverging very much. The light was exceedingly feeble, a perfectly dark room being required for its observation. When a few solid particles, as of dust or silk, were in the liquid, the brush was produced with much greater facility.

1453. The running together or coalescence of different lines of discharge (1412.) is very beautifully shown in the brush in air. This point may present a little difficulty to those who are not accustomed to see in every discharge an equal exertion of power in opposite directions, a positive brush being considered by such (perhaps in consequence of the common phrase *direction of a current*) as indicating a breaking forth in different directions of the original force, rather than a tendency to convergence and union in one line of passage. But the ordinary case of the brush may be compared, for its illustration, with that in which, by holding the knuckle opposite to highly excited glass, a discharge occurs, the ramifications of a brush then leading from the glass and converging into a spark on the knuckle. Though a difficult experiment to make, it is possible to obtain discharge between highly excited shell-lac and the excited glass of a machine: when the discharge passes, it is, from the nature of the charged bodies, brush at each end and spark in the middle, beautifully illustrating that tendency of discharge to facilitate like action, which I have described in a former page (1418.).

1454. The brush has *specific characters* in different gases, indicating a relation to the particles of these bodies even in a stronger degree than the spark (1422. 1423.). This effect is in strong contrast with the non-variation caused by the use of different substances as *conductors* from which the brushes are to originate. Thus, using such bodies as wood, card, charcoal, nitre, citric acid, oxalic acid, oxide of lead, chloride of lead, carbonate of potassa, potassa fusa, strong solution of potash, oil of vitriol, sulphur, sulphuret of antimony, and hæmatite, no variation in the character of the brushes was obtained, except that (dependent upon their effect as better or worse

conductors) of causing discharge with more or less readiness and quickness from the machine*.

1455. The following are a few of the effects I observed in different gases at the positively charged surfaces, and with atmospheres varying in their pressure. The general effect of rarefaction was the same for all the gases: at first, sparks passed; these gradually were converted into brushes, which became larger and more distinct in their ramifications, until, upon further rarefaction, the latter began to collapse and draw in upon each other, till they formed a stream across from conductor to conductor: then a few lateral streams shot out towards the glass of the vessel from the conductors; these became thick, flossy, and soft in appearance, and were succeeded by the full constant glow which covered the discharging wire. The phenomena varied with the size of the vessel (1477.), the degree of rarefaction, and the discharge of electricity from the machine. When the latter was in successive sparks, they were most beautiful, the effect of a spark from a small machine being equal to, and often surpassing, that produced by the *constant* discharge of a far more powerful one.

1456. *Air.*—Fine positive brushes are easily obtained in air at common pressures, and possess the well-known purplish light. When the air is rarefied, the ramifications are very long, filling the globe (1477.), the light is greatly increased, and is of a beautiful purple colour, with an occasional rose tint in it.

1457. *Oxygen.*—At common pressures, the brush is very close and compressed, and of a dull whitish colour. In rarefied oxygen, the form and appearance are better, the colour somewhat purplish, but all the characters very poor compared to those in air.

1458. *Nitrogen* gives brushes with great facility at the positive surface, far beyond any other gas I have tried: they are almost always fine in form, light, and colour, and in rarefied nitrogen are magnificent. They surpass the discharges in any other gas as to the quantity of light evolved.

1459. *Hydrogen*, at common pressures, gave a better brush than oxygen, but did not equal nitrogen; the colour was greenish grey. In rarefied hydrogen, the ramifications were very fine in form and distinctness, but pale in colour, with a soft and velvety appearance, and not at all equal to those in nitrogen. In the rarest state of the gas, the colour of the light was a pale gray green.

1460. *Coal gas.*—The brushes were rather difficult to produce, the contrast with nitrogen being great in this respect. They were short and strong, generally of a greenish colour, and possessing much of the spark character: for, occurring on both the positive and negative terminations, often when there was a dark interval of some length between the two brushes, still the quick, sharp sound of the spark was

* Exception must, of course, be made of those cases where the root of the brush, becoming a spark, causes a little diffusion or even decomposition of the matter there, and so gains more or less of a particular colour at that part.

produced, as if the discharge had been sudden through this gas, and partaking, in that respect, of the character of a spark. In rare coal gas, the forms were better, but the light very poor and the colour gray.

1461. *Carbonic acid gas* produces a very poor brush at common pressures, as regards either size, light, or colour; and this is probably connected with the tendency which this gas has to discharge the electricity as a spark (1422.). In rarefied carbonic acid, the brush is better in form, but weak as to light, being of a dull greenish or purplish hue, varying with the pressure and other circumstances.

1462. *Muriatic acid gas.*—It is very difficult to obtain the brush in this gas at common pressures. On gradually increasing the distance of the rounded ends, the sparks suddenly ceased when the interval was about an inch, and the discharge, which was still through the gas in the globe, was silent and dark. Occasionally a very short brush could for a few moments be obtained, but it quickly disappeared again. Even when the intermitting spark current (1455.) from the machine was used, still I could only with difficulty obtain a brush, and that very short, though I used rods with rounded terminations (about 0·25 of inch in diameter) which had before given them most freely in air and nitrogen. During the time of this difficulty with the muriatic gas, magnificent brushes were passing off from different parts of the machine into the surrounding air. On rarefying the gas, the formation of the brush was facilitated, but it was generally of a low squat form, very poor in light, and very similar on both the positive and negative surfaces. On rarefying the gas still more, a few large ramifications were obtained of a pale bluish colour, utterly unlike those in nitrogen.

1463. In all the gases, the different forms of disruptive discharge may be linked together and gradually traced from one extreme to the other, i. e. from the spark to the glow (1405.), or, it may be, to a still further condition to be called dark discharge; but it is, nevertheless, very surprising to see what a specific character each keeps whilst under the predominance of the general law. Thus, in muriatic acid, the brush is very difficult to obtain, and there comes in its place almost a dark discharge, partaking of the readiness of the spark action. Moreover, in muriatic acid, I have *never* observed the spark with any dark interval in it. In nitrogen, the spark readily changes its character into that of brush. In carbonic acid gas, there seems to be a facility to occasion spark discharge, whilst yet that gas is unlike nitrogen in the facility of the latter to form brushes, and unlike muriatic acid in its own facility to continue the spark. These differences add further force, first to the observations already made respecting the spark in various gases (1422. 1423.), and then, to the proofs deducible from it, of the relation of the electrical forces to the particles of matter.

1464. The peculiar characters of nitrogen in relation to the electric discharge (1422. 1458.) must, evidently, have an important influence over the form and even the occurrence of lightning. Being that gas which most readily produces coruscа-

tions, and, by them, extends discharge to a greater distance than any other gas tried, it is also that which constitutes four fifths of our atmosphere; and as, in atmospheric electrical phenomena, one, and sometimes both the inductive forces are resident on the particles of the air, which, though probably affected as to conducting power by the aqueous particles in it, cannot be considered as a good conductor, so the peculiar power possessed by nitrogen, to originate and effect discharge in the form of a brush or of ramifications, has, probably, an important relation to its electrical service in nature, as it most seriously affects the character and condition of the discharge when made. The whole subject of discharge from and through gases is a most important one to science, and, if only in reference to atmospheric electricity, deserves extensive and close experimental investigation.

Difference of discharge at the positive and negative conducting surfaces.

1465. I have avoided speaking of this well-known phenomenon more than was quite necessary, that I might bring together here what I have to say on the subject. When the brush discharge is observed in air at the positive and negative surfaces, there is a very striking difference, the true and full comprehension of which would, no doubt, be of the utmost importance to the physics of electricity; it would throw great light on our present subject, i. e. the molecular action of dielectrics under induction, and its consequences, and seems very open to, and accessible by, experimental inquiry.

1466. The difference in question used to be expressed in former times by saying, that a point charged positively gave brushes into the air, whilst the same point charged negatively gave a star. This is true only of bad conductors, or of metallic conductors charged intermittingly, or otherwise controlled by collateral induction. If metallic points project *freely* into the air, the positive and negative light upon them differ very little in appearance, and the difference can be observed only upon close examination.

1467. The effect varies exceedingly under different circumstances, but, as we must set out from some position, may perhaps be stated thus: if a metallic wire with a rounded termination in free air be used to produce the brushy discharge, then the brushes obtained when the wire is charged negatively are very poor and small, by comparison with those produced when the charge is positive. Or if a large metal ball connected with the electrical machine be charged *positively*, and a fine uninsulated point be gradually brought towards it, a star appears on the point when at a considerable distance, which, though it becomes brighter, does not change its form of a star until it is close up to the ball: whereas, if the ball be charged negatively, the point at a considerable distance has a star on it as before; but when brought nearer, (in my case to the distance of $1\frac{1}{2}$ inches,) a brush formed on it, extending to the negative ball; and when still nearer, (at $\frac{1}{8}$ of an inch distance,) the brush ceased, and bright sparks passed. These variations, I believe, include the whole series of differ-

ences, and they seem to show at once, that the negative surface tends to retain its discharging character unchanged, whilst the positive surface, under similar circumstances, permits of great variation.

1468. There are several points in the character of the negative discharge to air which it is important to observe. A metal rod, 0·3 of an inch in diameter, with a rounded end projecting into the air, was charged negatively, and gave a short noisy brush (fig. 8.). It was ascertained both by sight (1427. 1433.) and sound (1431.), that the successive discharges were very rapid in their recurrence, being seven or eight times more numerous in the same period, than those produced when the rod was charged positively to an equal degree. When the rod was positive, it was easy, by working the machine a little quicker, to replace the brush by a glow (1405. 1463.), but when it was negative no efforts could produce this change. Even by bringing the hand opposite the wire, the only effect was to increase the number of brush discharges in a given period, raising at the same time the sound to a higher pitch.

1469. A point opposite the negative brush exhibited a star, and as it was approximated caused the size and sound of the negative brush to diminish, and, at last, to cease, leaving the negative end silent and dark, yet effective as to discharge.

1470. When the round end of a smaller wire (fig. 9.) was advanced towards the negative brush, it (becoming positive by induction) exhibited the quiet glow at 8 inches distance, the negative brush continuing. When nearer, the pitch of the sound of the negative brush rose, indicating quicker intermittences (1431.); still nearer, the positive end threw off ramifications and distinct brushes; at the same time, the negative brush contracted in its lateral directions and collected together, giving a peculiar narrow longish brush, in shape like a hair pencil, the two brushes existing at once, but very different in their form and appearance, and especially in the more rapid recurrence of the negative discharges than of the positive. On using a smaller positive wire for the same experiment, the glow first appeared on it, and then the brush, the negative brush being affected at the same time; and the two at one distance became exceedingly alike in appearance, and the sounds, I thought, were in unison; at all events they were in harmony, so that the intermissions of discharge were either isochronous, or a simple ratio existed between the intervals. With a higher action of the machine, the wires being retained unaltered, the negative surface would become dark and silent, and a glow appear on the positive one. A still higher action changed the latter into a spark. Finer positive wires gave other variations of these effects, which I must not allow myself to go into here.

1471. A thinner rod was now connected with the negative conductor in place of the larger one (1468.), its termination being gradually diminished to a blunt point, as in fig. 10.; and it was beautiful to observe that, notwithstanding the variation of the brush, the same general order of effects was produced. The end gave a small sonorous negative brush, which the approach of the hand or a large conducting surface did not alter, until it was so near as to produce a spark. A fine point oppo-

site to it was luminous at a distance; being nearer it did not destroy the light and sound of the negative brush, but only tended to have a brush produced on itself, which, at a still nearer distance, passed into a spark joining the two surfaces.

1472. When the distinct negative and positive brushes are produced simultaneously in relation to each other in air, the former almost always has a contracted form, as in fig. 11., very much indeed resembling the figure which the positive brush itself has when influenced by the lateral vicinity of positive parts acting by induction. Thus a brush issuing from a point in the re-entering angle of a positive conductor has the same compressed form (fig. 12.).

1473. The character of the negative brush is not affected by the chemical nature of the substances of the conductors (1454.), but only by their possession of the conducting power in a greater or smaller degree.

1474. Rarefaction of common air about a negative ball or blunt point facilitated the development of the negative brush, the effect being, I think, greater than on a positive brush, though great on both. Extensive ramifications could be obtained from a ball or end electrified negatively to the plate of the air-pump on which the jar containing it stood.

1475. A very important variation of the relative forms and conditions of the positive and negative brush takes place on varying the dielectric in which they are produced. The difference is so very great that it points to a specific relation of this form of discharge to the particular gas in which it takes place, and opposes the idea that gases are but obstructions to the discharge, acting one like another and merely in proportion to their pressure (1377.).

1476. In *air*, the superiority of the positive brush is well known (1467. 1472.). In *nitrogen*, it is as great or even greater than in air (1458.). In *hydrogen*, the positive brush loses a part of its superiority, not being so good as in nitrogen or air; whilst the negative brush does not seem injured (1459.). In *oxygen*, the positive brush is compressed and poor (1457.); whilst the negative did not sink in character: the two were so alike that the eye frequently could not tell the one from the other, and this similarity continued when the oxygen was gradually rarefied. In *coal gas* the brushes are difficult of production as compared to nitrogen (1460.), and the positive not much superior to the negative in its character, either at common or low pressures. *In carbonic acid gas*, this approximation of character also occurred. In *muriatic acid gas* the positive brush was very little better than the negative, and both difficult to produce (1462.) as compared with the facility in nitrogen or air.

1477. These experiments were made with rods of brass about a quarter of an inch thick having rounded ends, the ends being opposed in a glass globe 7 inches in diameter, containing the gas to be experimented with. The electric machine was used to communicate directly, sometimes the positive, and sometimes the negative, state, to the rod in connection with it.

1478. Thus we see that, notwithstanding there is a general difference in favour of the superiority of the positive brush over the negative, that difference is at its maximum in nitrogen and air; whilst in carbonic acid, muriatic acid, coal gas, and oxygen it diminishes, and at last becomes almost nothing. So that in this particular effect, as in all others yet examined, the evidence is in favour of that view which refers the results to a direct relation of the electric forces with the molecules of the matter concerned in the action (1421. 1423. 1463.). Even when special phenomena arise under the operation of the general law, the theory adopted seems fully competent to meet the case.

1479. Before I proceed further in tracing the probable cause of the difference between the positive and negative brush discharge, I wish to know the results of a few experiments which are in course of preparation: and thinking this Series of Researches long enough, I shall here close it with the expectation of being able in a few weeks to renew the inquiry, and entirely redeem my pledge (1306.).

Royal Institution,
Dec. 23rd, 1837.

VIII. *Experimental Researches in Electricity.—Thirteenth Series.* By MICHAEL FARADAY, *Esq., D.C.L. F.R.S. Fullerian Prof. Chem. Royal Institution, Corr. Memb. Royal and Imp. Acadd. of Sciences, Paris, Petersburgh, Florence, Copenhagen, Berlin, &c. &c.*

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§. 18. *On Induction (continued).* ¶ ix. *Disruptive discharge (continued)—Peculiarities of positive and negative discharge either as spark or brush—Glow discharge—Dark discharge.*—¶ x. *Convection, or carrying discharge.* ¶ xi. *Relation of a vacuum to electrical phenomena.* §. 19. *Nature of the electrical current.*

¶ ix. *Disruptive discharge (continued).*

1480. LET us now direct our attention to the general difference of the positive and negative disruptive discharge, with the object of tracing, as far as possible, the cause of that difference, and whether it depends on the charged conductors principally, or on the interposed dielectric; and as it appears to be great in air and nitrogen (1476.), let us observe the phenomena in air first.

1481. The general case is best understood by a reference to surfaces of considerable size rather than to points, which involve (as a secondary effect) the formation of currents (1562.). My investigation, therefore, was carried on with balls and terminations of different diameters, and the following are some of the principal results.

1482. If two balls of very different dimensions, as for instance one, half an inch, and the other three inches, in diameter, be arranged at the ends of rods so that either can be electrified by a machine and made to discharge by sparks to the other, which is at the same time uninsulated; then, as is well known, far longer sparks are obtained when the small ball is positive and the large ball negative, than when the small ball is negative and the large ball positive. In the former case, the sparks are 10 or 12 inches in length; in the latter an inch or an inch and a half only.

1483. But previous to the description of further experiments, I will mention two words, for which with many others I am indebted to a friend, and which I think it would be expedient to introduce and use. It is important in ordinary inductive action, to distinguish at which charged surface the induction originates and is sustained: i. e. if two or more metallic balls, or other masses of matter, are in inductive relation, to express which are charged originally, and which are brought by them into the

opposite electrical condition. I propose to call those bodies which are originally charged, *inductric* bodies; and those which assume the opposite state, in consequence of the induction, *inducteous* bodies. This distinction is not needful because there is any difference between the sums of the *inductric* and the *inducteous* forces; but principally because, when a ball A is inductric, it not merely brings a ball B, which is opposite to it, into an inducteous state, but also many other surrounding conductors, though some of them may be a considerable distance off, and the consequence is, that the balls do not bear the same precise relation to each other when, first the one, and then the other, is made the inductric ball; though, in each case, the *same ball* be made to assume the *same state*.

1484. Another liberty which I may also occasionally take in language I will explain and limit. It is that of calling a particular spark or brush, *positive* or *negative*, according as it may be considered as *originating* at a positive or a negative surface. We speak of the brush as positive or negative when it shoots out from surfaces previously in those states; and the experiments of Mr. WHEATSTONE go to prove that it *really begins* at the charged surface, and from thence extends into the air (1437. 1438.) or other dielectric. According to my view, *sparks* also originate or are determined at one particular spot (1370.), namely, that where the tension first rises up to the maximum degree; and when this can be determined, as in the simultaneous use of large and small balls, in which case the discharge begins or is determined by the latter, I would call that discharge which passes *at once*, a positive spark, if it was at the positive surface that the maximum intensity was first obtained, or a negative spark, if that necessary intensity was first obtained at the negative surface.

1485. An apparatus was arranged, as in fig. 15. (Plate III.): A and B, were brass balls of very different diameters attached to metal rods, moving through sockets on insulating pillars, so that the distance between the balls could be varied at pleasure. The large ball A, 2 inches in diameter, was connected with an insulated brass conductor, which could be rendered positive or negative directly from a cylinder machine: the small ball B, 0·25 of an inch in diameter, was connected with a discharging train (292.) and perfectly uninsulated. The brass rods sustaining the balls were 0·2 of an inch in thickness.

1486. When the large ball was *positive* and inductric (1483.), negative sparks occurred until the interval was 0·49 of an inch; then mixed brush and spark between that and 0·51; and from 0·52 and upwards, negative brush alone. When the large ball was made *negative* and inductric, then positive spark alone occurred until the interval was as great as 1·15 inches; spark and brush from that up to 1·55; and to have the positive brush alone, it required an interval of at least 1·65 inches.

1487. The balls A and B were now changed for each other. Then making the small ball B inductric *positively*, the positive sparks alone continued only up to 0·67; spark and brush occurred from 0·68 up to 0·72; and positive brush alone from 0·74 and upwards. Rendering the small ball B inductric and *negative*, negative sparks

alone occurred up to 0·40; then spark and brush at 0·42; whilst from 0·44 and upwards the noisy negative brush alone took place.

1488. We thus find a great difference as the balls are rendered inductric or inducteous; the small ball rendered *positive* inducteously giving a spark nearly twice as long as that produced when it was charged positive inductrically, and a similar difference, though not, under the circumstances, to the same extent, was manifest when it was rendered *negative*.

1489. Another result is, that the small ball rendered positive gives a much longer spark than when it is rendered negative, and that the small ball rendered negative gives a brush more readily than when positive, in relation to the effect of increasing distance.

1490. When the interval was below 0·4 of an inch, so that the small ball should give sparks, whether positive or negative, I could not observe that there was any constant difference, either in their ready occurrence or the number which passed in a given time. But when the interval was such that the small ball when negative gave a brush, then the discharges from it, as separate negative brushes, were far more numerous than the corresponding discharges from it when rendered positive, whether those positive discharges were as sparks or brushes.

1491. It is, therefore, evident that, when a ball is discharging electricity in the form of brushes, the brushes are far more numerous, and each contains or carries off far less electric force when the electricity so discharged is negative, than when it is positive.

1492. In all such experiments as those described, the point of change from spark to brush is very much governed by the working state of the electrical machine and the size of the conductor connected with the discharging ball. If the machine be in strong action and the conductor large, so that much power is accumulated quickly for each discharge, then the interval is greater at which the sparks are replaced by brushes; but the general effect is the same.

1493. These results, though indicative of very striking and peculiar relations of the electric force or forces, do not show the relative degrees of charge which the small ball acquires before discharge occurs, i. e. they do not tell whether it acquires a higher condition in the negative, or in the positive state, immediately preceding that discharge. To illustrate this important point I arranged two places of discharge as represented, fig. 16. A and D, are brass balls 2 inches in diameter, B and C are smaller brass balls 0·25 of an inch in diameter; the forks L and R supporting them were of brass wire 0·2 of an inch in diameter: the space between the large and small ball on the same fork was 5 inches, that the two places of discharge *n* and *o* might be sufficiently removed from each other's influence. The fork L was connected with a projecting cylindrical conductor, which could be rendered positive or negative at pleasure, by an electrical machine, and the fork R was attached to another conductor, but thrown into an uninsulated state by connection with a dis-

charging train. The two intervals or places of discharge *n* and *o* could be varied at pleasure, their extent being measured by the occasional introduction of a diagonal scale. It is evident, that, as the balls A and B connected with the same conductor are always charged at once, and that discharge may take place to either of the balls connected with the discharging train, the intervals of discharge *n* and *o* may be properly compared to each other, as respects the influence of large and small balls when charged positively and negatively in air.

1494. When the intervals *n* and *o* were each made = 0·9 of an inch, and the balls A and B inductric *positively*, the discharge was all at *n* from the small ball of the conductor to the large ball of the discharging train, and mostly by positive brush, though once by a spark. When the balls A and B were made inductric *negatively*, the discharge was still from the same small ball, at *n*, by a constant negative brush.

1495. I diminished the intervals *n* and *o* to 0·6 of an inch. When A and B were inductric *positively*, all the discharge was at *n* as a positive brush: when A and B were inductric *negatively*, still all the discharge was at *n*, as a negative brush.

1496. The facility of discharge at the positive and negative small balls, therefore, did not appear to be very different. If a difference had existed, there were always two small balls, one in each state, that the discharge might happen at that most favourable to the effect. The only difference was, that one was in the inductric, and the other in the inducteous state, but whichever happened for the time to be in that state, whether positive or negative, had the advantage.

1497. To counteract this interfering influence, I made the interval *n* = 0·79 and interval *o* = 0·58 of an inch. Then, when the balls A and B were *inductric positive*, the discharge was about equal at the two intervals. When, on the other hand, the balls A and B were inductric *negative*, there was discharge, still at both, but most at *n*, as if the small ball *negative* could discharge a little easier than the same ball *positive*.

1498. The small balls and terminations used in these and similar experiments may very correctly be compared, in their action, to the same balls and ends when electrified in free air at a much greater distance from conductors, than they were in those cases from each other. In the first place, the discharge, even when as a spark, is, according to my view, determined, and, so to speak, begins at a spot on the surface of the small ball (1374.), occurring when the intensity there has risen up to a certain maximum limiting degree (1370.); this determination of discharge at a particular spot first, being easily traced from the spark into the brush, by increasing the distance, so as, at last, even to render evident the time which is necessary (1436. 1438.). In the next place, the large balls which I have used might be replaced by larger balls at a still greater distance, and so, by successive degrees, may be considered as passing into the sides of the rooms; these being under general circumstances the inducteous bodies, whilst the small ball rendered either positive or negative is the inductric body.

1499. But, as has long been recognised, the small ball is only a blunt end, and, electrically speaking, a point only a small ball; so that when a point or blunt end is

throwing out its brushes into the air, it is acting exactly as the small balls have acted in the experiments already described, and by virtue of the same properties and relations.

1500. It may very properly be said with respect to the experiments, that the large negative ball is as essential to the discharge as the small positive ball, and also that the large negative ball shows as much superiority over the large positive ball (which is inefficient in causing a spark from its opposed small negative ball) as the small positive ball does over the small negative ball; and probably when we understand the real cause of the difference, and refer it rather to the condition of the particles of the dielectric than to the sizes of the conducting balls, we may find much importance in such an observation. But for the present, and whilst engaged in investigating the point, we may admit, what is the fact, that the forces are of higher intensity at the surfaces of the smaller balls than at those of the larger (1372. 1374.); that the former, therefore, determine the discharge, by first rising up to that exalted condition which is necessary for it; and that, whether brought to this condition by induction towards the walls of a room or the large balls I have used, these may fairly be compared one with the other in their influence and actions.

1501. The conclusions I arrive at are: first, that when two equal small conducting surfaces equally placed in air are electrified, one positively and the other negatively, that which is negative can discharge to the air at a tension a little lower than that required for the positive ball: second, that when discharge does take place, much more passes at each time from the positive than from the negative surface (1491.). The last conclusion is very abundantly proved by the optical analysis of the positive and negative brushes already described (1468.), the latter set of discharges being found to recur five or six times oftener than the former*.

1502. If, now, a small ball be made to give brushes or brushy sparks by a powerful machine, we can, in some measure, understand and relate the difference perceived when it is rendered positive or negative. It is known to give when positive a much larger and more powerful spark than when negative, and with greater facility (1482.); in fact, the spark, although it takes away so much more electricity at once, commences at a tension higher only in a small degree, if at all. On the other hand, if rendered negative, though discharge may commence at a lower degree, it continues but for a very short period, very little electricity passing away each time. These circumstances are directly related, for the extent to which the positive spark can reach, and the size and extent of the positive brush, are consequences of the capability which exists of much electricity passing off at one discharge from the positive surface (1468. 1501.).

1503. But to relate these effects only to the form and size of the conductor, would, according to my notion of induction, be a very imperfect mode of viewing the whole

* A very excellent mode of examining the relation of small positive and negative surfaces would be by the use of drops of gum water, solutions, or other liquids. See onwards (1581. 1593.).

question (1523.). I expect that the effects are due altogether to the mode in which the particles of the interposed dielectric polarize, and I have already given some experimental indications of the differences presented by different dielectrics in this respect (1475. 1476.). The modes of polarization, as I shall have occasion hereafter to show, may be very diverse in different dielectrics. With respect to common air, what seems to be the consequence of a superiority in the positive force at the surface of the small ball, may be due to the more exalted condition of the negative polarity of the particles of air, or of the nitrogen in it (the negative part being, perhaps, more compressed, whilst the positive part is more diffuse, or *vice versâ*); for such a condition could determine certain effects at the positive ball which would not take place to the same degree at the negative ball, just as well as if the positive ball had possessed some special and independent power of its own.

1504. That the effects are more likely to be dependent upon the dielectric than the ball, is supported by the character of the two discharges. If a small positive ball be throwing off brushes with ramifications ten inches long, how can the ball affect that part of a ramification which is five inches from it? Yet the portion beyond that place has the same character as that preceding it, and no doubt has that character impressed by the same general principle and law. Looking upon the action of the contiguous particles of a dielectric as fully proved, I see, in such a ramification, a propagation of discharge from particle to particle, each doing for the one next it what was done for it by the preceding particle, and what was done for the first particle by the charged metal against which it was situated.

1505. With respect to the general condition and relations of the positive and negative brushes in dense or rare air, or in other media and gases, if they are produced at different times and places, they are of course independent of each other. But when they are produced from opposed ends or balls at the same time, in the same vessel of gas (1470. 1477.), they are frequently related; and circumstances may be so arranged that they shall be isochronous, occurring in equal numbers in equal times; or shall occur in multiples, i. e. with two or three negatives to one positive; or shall alternate, or be quite irregular. All these variations I have witnessed; and when it is considered that the air in the vessel, and also the glass of the vessel, can take a momentary charge, it is easy to comprehend their general nature and cause.

1506. Similar experiments to those in air (1485. 1493.) were made in different gases, the results of which I will describe as briefly as possible. The apparatus is represented fig. 17. consisting of a bell-glass eleven inches in diameter at the widest part, and ten and a half inches high up to the bottom of the neck. The balls are lettered, as in fig. 16, and are in the same relation to each other; but A and B were on separate sliding wires, which, however, were generally joined by a cross wire, *w*, above, and that connected with the brass conductor, which received its positive or negative charge from the machine. The rods of A and B were graduated at the part moving through the stuffing-box, so that the application of a diagonal scale applied there, told what was

the distance between these balls and those beneath them. As to the position of the balls in the jar, and their relation to each other, C and D were three and a quarter inches apart, their height above the pump plate five inches, and the distance between any of the balls and the glass of the jar one and three quarter inches at least, and generally more. The balls A and D were two inches in diameter, as before (1493.): the balls B and C only 0·15 of an inch in diameter.

Another apparatus was occasionally used in connection with the one just described, being an open discharger (fig. 18.), by which a comparison of the discharge in air and that in gases could be obtained. The balls E and F, each 0·6 of an inch in diameter, were connected with sliding rods and other balls, and were insulated. When used for comparison, the brass conductor was associated at the same time with the balls A and B of figure 17 and ball E of this apparatus (fig. 18.); whilst the balls C, D and F were connected with the discharging train.

1507. I will first tabulate the results as to the *restraining power* of the gases over discharge. The balls A and C (fig. 17.) were thrown out of action by distance, and the effects at B and D, or the interval n in the gas, compared with those at the interval p in the air, between E and F (fig. 18.). The Table sufficiently explains itself. It will be understood, that all discharge was in the air, when the interval there was less than that expressed in the first or third columns of figures; and all the discharge in the gas, when the interval in air was greater than that in the second or fourth column of figures. At intermediate distances the discharge was occasionally at both places, i. e. sometimes in the air, sometimes in the gas.

	Interval p in parts of an inch.			
Constant interval n between B and D = 1 inch.	When the small ball B was inductric and *positive* the discharge was all at p in air before.	at n in the gas after.	When the small ball B was inductric and *negative* the discharge was all at p in air before.	at n in the gas after.
	$p =$	$p =$	$p =$	$p =$
In Air	0·40	0·50	0·28	0·33
In Nitrogen	0·30	0·65	0·31	0·40
In Oxygen	0·33	0·52	0·27	0·30
In Hydrogen	0·20	0·40	0·22	0·24
In Coal gas	0·20	0·90	0·20	0·27
In Carbonic acid	0·64	1·30	0·30	0·45

1508. These results are the same generally, as far as they go, as those of the like nature in the last series (1388.), and confirm the conclusion that different gases restrain discharge in very different proportions. They are probably not so good as the former ones, for the glass jar not being varnished, acted irregularly, sometimes taking a certain degree of charge as a non-conductor, and at other times acting as a conductor in the conveyance and derangement of that charge. Another cause of difference in the ratios is, no doubt, the relative sizes of the discharge balls in air; in the former case they were of very different size, here they were alike.

1509. In future experiments intended to have the character of accuracy, the influence of these circumstances ought to be ascertained, and, above all things, the gases themselves ought to be contained in vessels of metal, and not of glass.

1510. The next set of results are those obtained when the intervals *n* and *o* (fig. 17.) were made equal to each other, and relate to the greater facility of discharge at the small ball, when rendered positive or negative (1493.).

1511. In *air*, with the intervals = 0·4 of an inch, A and B being inductric and positive, discharge was nearly equal at *n* and *o*; when A and B were inductric and negative, the discharge was mostly at *n* by negative brush. When the intervals were = 0·8 of an inch, with A and B inductric positively, all discharge was at *n* by positive brush; with A and B inductric negatively, all the discharge was at *n* by a negative brush. It is doubtful, therefore, from these results, whether the negative ball has any greater facility than the positive.

1512. *Nitrogen.*—Intervals *n* and *o* = 0·4 of an inch: A, B inductric positive, discharge at both intervals, most at *n*, by positive sparks; A, B inductric negative, discharge equal at *n* and *o*. The intervals made = 0·8 of an inch: A, B inductric positive, discharge all at *n* by positive brush; A, B inductric negative, discharge most at *o* by positive brush. In this gas, therefore, though the difference is not decisive, it would seem that the positive small ball caused the most ready discharge.

1513. *Oxygen.*—Intervals *n* and *o* = 0·4 of an inch: A, B inductric positive, discharge nearly equal; inductric negative, discharge mostly at *n* by negative brush. Made the intervals = 0·8 of an inch: A, B inductric positive, discharge at *n* and *o*; inductric negative, discharge all at *o* by negative brush. So here the negative small ball seems to give the most ready discharge.

1514. *Hydrogen.*—Intervals *n* and *o* = 0·4 of an inch: A, B inductric positive, discharge nearly equal; inductric negative, discharge mostly at *o*. Intervals = 0·8 of an inch: A and B inductric positive, discharge mostly at *n*, as positive brush; inductric negative, discharge mostly at *o*, as positive brush. Here the positive discharge seems most facile.

1515. *Coal gas.*—*n* and *o* = 0·4 of an inch: A, B inductric positive, discharge nearly all at *o* by negative spark: A, B inductric negative, discharge nearly all at *n* by negative spark. Intervals = 0·8 of an inch, and A, B inductric positive, discharge mostly at *o* by negative brush: A, B inductric negative, discharge all at *n* by negative brush. Here the negative discharge most facile.

1516. *Carbonic acid gas.*— *n* and *o* = 0·4 of an inch: A, B inductric positive, discharge nearly all at *o*, or negative: A, B inductric negative, discharge nearly all at *n*, or negative. Intervals = 0·8 of an inch: A, B inductric positive, discharge mostly at *o*, or negative: A, B inductric negative, discharge all at *n*, or negative. In this case the negative had a decided advantage in facility of discharge.

1517. Thus, if we may trust this form of experiment, the negative small ball has a decided advantage in facilitating disruptive discharge over the positive small ball in

some gases, as in carbonic acid gas and coal gas (1399.), whilst in others that conclusion seems more doubtful; and in others, again, there seems a probability that the positive small ball may be superior. All these results were obtained at very nearly the same pressure of the atmosphere.

1518. I made some experiments in these gases whilst in the air jar (fig. 17.), as to the change from spark to brush, analogous to those in the open air already described (1486. 1487.). I will give, in a Table, the results as to when brush began to appear mingled with the spark; but the after results were so varied, and the nature of the discharge in different gases so different, that to insert the results obtained without further investigation, would be of little use. At intervals less than those expressed the discharge was always by spark.

	Discharge between balls B and D.		Discharge between balls A and C.	
	Small ball B inductric *pos.*	Small ball B inductric *neg.*	Large ball A inductric *pos.*	Large ball A inductric *neg.*
Air	0·55	0·30	0·40	0·75
Nitrogen.....	0·30	0·40	0·52	0·41
Oxygen	0·70	0·30	0·45	0·82
Hydrogen	0·20	0·10		
Coal gas	0·13	0·30	0·30	0·14
Carbonic acid..	0·82	0·43	1·60	above 1·80; had not space.

1519. It is to be understood that sparks occurred at much higher intervals than these; the table only expresses that distance beneath which all discharge was as spark. Some curious relations of the different gases to discharge are already discernible, but it would be useless to consider them until illustrated by further experiments.

1520. I ought not to omit noticing here, that Professor Belli of Milan has published a very valuable set of experiments on the relative dissipation of positive and negative electricity in the air*; he finds the latter far more ready, in this respect, than the former.

1521. I made some experiments of a similar kind, but with sustained high charges; the results were less striking than those of Signore Belli, and I did not consider them as satisfactory. I may be allowed to mention, in connection with the subject, an interfering effect which embarrassed me for a long time. When I threw positive electricity from a given point into the air, a certain intensity was indicated by an electrometer on the conductor connected with the point, but as the operation continued this intensity rose several degrees; then making the conductor negative with the same point attached to it, and all other things remaining the same, a certain degree of tension was observed in the first instance, which also gradually rose as the operation proceeded. Returning the conductor to the positive state, the tension was at first low, but rose as before; and so also when again made negative.

* Bibliotheque Universelle, 1836, September, p. 152.

1522. This result appeared to indicate that the point which had been giving off one electricity, was, by that, more fitted for a short time to give off the other. But on closer examination I found the whole depended upon the inductive reaction of that air, which being charged by the point, and gradually increasing in quantity before it, as the positive or negative issue was continued, diverted and removed a part of the inductive action of the surrounding wall, and thus apparently affected the powers of the point, whilst really it was the dielectric itself that was causing the change of tension.

1523. The results connected with the different conditions of positive and negative discharge will have a far greater influence on the philosophy of electrical science than we at present imagine, especially if, as I believe, they depend on the peculiarity and degree of polarized condition which the molecules of the dielectrics concerned acquire (1503. 1600.). Thus, for instance, the relation of our atmosphere and the earth within it, to the occurrence of spark or brush, must be especial and not accidental. It would not else consist with other meteorological phenomena, also of course dependent on the special properties of the air, and which being themselves in harmony the most perfect with the functions of animal and vegetable life, are yet restricted in their actions, not by loose regulations, but by laws the most precise.

1524. Even in the passage through air of the voltaic current, we see the peculiarities of positive and negative discharge at the two charcoal points; and if these discharges are made to take place simultaneously to mercury, the distinction is still more remarkable.

1525. It seems very possible that the striking difference recently observed and described by my friend Professor DANIELL*, namely, that when a zinc and a copper ball, the same in size, were placed respectively in copper and zinc spheres, also the same in size, and excited by electrolytes or dielectrics of the same strength and nature, the zinc ball far surpassed the zinc sphere in action, may also be connected with these phenomena; for it is not difficult to conceive how the polarity of the particles shall be affected by the circumstance of the positive surface, namely the zinc, being the larger or the smaller of the two inclosing the electrolyte. It is even possible, that with different electrolytes or dielectrics the ratio may be considerably varied, or in some cases even inverted.

Glow discharge.

1526. That form of disruptive discharge which appears as a *glow* (1359. 1405.), is very peculiar and beautiful: it seems to depend on a quick and almost continuous charging of the air close to, and in contact with, the conductor.

1527. *Diminution of the charging surface* will produce it. Thus, when a rod 0·3

* Philosophical Transactions, 1838, p. 47.

of an inch in diameter, with a rounded termination, was rendered positive in free air, it gave fine brushes from the extremity, but occasionally these disappeared, and a quiet phosphorescent continuous glow took their place, covering the whole of the end of the wire, and extending a very small distance from the metal into the air. With a rod 0·2 of an inch in diameter the glow was more readily produced. With still smaller rods, and also with blunt conical points, it occurred still more readily; and with a fine point I could not obtain the brush in free air, but only this glow. The positive glow and the positive star are, in fact, the same.

1528. *Increase of power in the machine* tends to produce the glow; for rounded terminations which will give only brushes where the machine is in weak action, will readily give the glow when it is in good order.

1529. *Rarefaction of the air* wonderfully favours the glow phenomena. A brass ball, two and a half inches in diameter, being made positively inductric in an air-pump receiver, became covered with glow over an area of two inches in diameter, when the pressure was reduced to 4·4 inches of mercury. By a little adjustment the ball could be covered all over with this light. Using a brass ball 1·25 inches in diameter, and making it inducteously positive by an inductric negative point, the phenomena, at high degrees of rarefaction, were exceedingly beautiful. The glow came over the positive ball, and gradually increased in brightness, until it was at last very luminous; and it also stood up like a low flame, half an inch or more in height. On touching the sides of the glass jar this lambent flame was affected, assumed a ring form, like a crown on the top of the ball, appeared flexible, and revolved with a comparatively slow motion, i. e. about four or five times in a second. This ring-shape and revolution are beautifully connected with the mechanical currents (1576.) taking place within the receiver. These glows in rarefied air are often highly exalted in beauty by a spark discharge at the conductor (1551. *Note*).

1530. To obtain a *negative glow* in air at common pressures is difficult. I did not procure it on the rod 0·3 of an inch in diameter by my machine, nor on much smaller rods; and it is questionable as yet, whether, even on fine points, what is called the negative star is a very reduced and minute, but still intermitting brush, or a glow similar to that obtained on a positive point.

1531. In rarefied air the negative glow can easily be obtained. If the rounded ends of two metal rods, about 0·2 of an inch in diameter, are introduced into a globe or jar (the air within being rarefied), and being opposite to each other, are about four inches apart, the glow can be obtained on both rods, covering not only the ends, but an inch or two of the part behind. On using *balls* in the air-pump jar, and adjusting the distance and exhaustion, the negative ball could be covered with glow, whether it were the inductric or the inducteous surface.

1532. When rods are used it is necessary to be aware that, if placed concentrically in the jar or globe, the light on one rod is often reflected by the sides of the vessel on to the other rod, and makes it apparently luminous, when really it is not so. This

effect may be detected by shifting the eye at the time of observation, or avoided by using blackened rods.

1533. It is curious to observe the relation of glow, brush, and spark to each other, as produced by positive or negative surfaces; thus, beginning with spark discharge, it passes into brush much sooner when the surface at which the discharge commences (1484.) is negative, than it does when positive; but proceeding onwards in the order of change, we find that the positive brush passes into *glow* long before the negative brush does. So that, though each presents the three conditions in the same general order, the series are not precisely the same. It is probable, that, when these points are minutely examined, as they must be shortly, we shall find that each different gas or dielectric presents its own peculiar results, dependent upon the mode in which its particles assume polar electric condition.

1534. The glow occurs in all gases in which I have looked for it. These are air, nitrogen, oxygen, hydrogen, coal gas, carbonic acid, muriatic acid, sulphurous acid and ammonia. I thought also that I obtained it in oil of turpentine, but if so it was very dull and small.

1535. The glow is always accompanied by a wind proceeding either directly out from the glowing part, or directly towards it; the former being the most general case. This takes place even when the glow occurs upon a ball of considerable size: and if matters be so arranged that the ready and regular access of air to a part exhibiting the glow be interfered with or prevented, the glow then disappears.

1536. I have never been able to analyse or separate the glow into visible elementary intermitting discharges (1427. 1433.), nor to obtain the other evidence of intermitting action, namely an audible sound (1431.). The want of success, as respects trials made by ocular means, may depend upon the large size of the glow preventing the separation of the visible images: and, indeed, if it does intermit, it is not likely that all parts intermit at once with a simultaneous regularity.

1537. All the effects tend to show, that *glow* is due to a continuous charge or discharge of air; in the former case being accompanied by a current from, and in the latter by one to, the place of the glow. As the surrounding air comes up to the charged conductor, on attaining that spot at which the tension of the particles is raised to the sufficient degree (1370. 1410.), it becomes charged, and then moves off, by the joint action of the forces to which it is subject; and, at the same time that it makes way for other particles to come and be charged in turn, actually helps to form that current by which they are brought into the necessary position. Thus, through the regularity of the forces, a constant and quiet result is produced; and that result is, the charging of successive portions of air, the production of a current, and of a continuous glow.

1538. I have frequently been able to make the termination of a rod, which, when left to itself, would produce a brush, produce in preference a glow, simply by aiding the formation of a current of air at its extremity; and, on the other hand, it is not at all

difficult to convert the glow into brushes, by affecting the current of air (1574. 1579.) or the inductive action near it.

1539. The transition from glow, on the one hand, to brush and spark, on the other, and, therefore, their connexion, may be established in various ways. Those circumstances which tend to facilitate the charge of the air by the excited conductor, and also those which tend to keep the tension at the same degree notwithstanding the discharge, assist in producing the glow; whereas those which tend to resist the charge of the air or other dielectric, and those which favour the accumulation of electric force prior to discharge, which, sinking by that act, has to be exalted before the tension can again acquire the requisite degree, favour intermitting discharge, and, therefore, the production of brush or spark. Thus, rarefaction of the air, the removal of large conducting surfaces from the neighbourhood of the glowing termination, the presentation of a sharp point towards it, help to sustain or produce the glow: but the condensation of the air, the presentation of the hand or other large surface, the gradual approximation of a discharging ball, tend to convert the glow into brush or even spark. All these circumstances may be traced and reduced, in a manner easily comprehensible, to their relative power of assisting to produce, either a *continuous* discharge to the air, which gives the glow; or an interrupted one, which produces the brush, and, in a more exalted condition, the spark.

1540. The rounded end of a brass rod, 0·3 of an inch in diameter, was covered with a positive glow by the working of an electrical machine: on stopping the machine, so that the charge of the connected conductor should fall, the glow changed for a moment into brushes just before the discharge ceased altogether, illustrating the necessity for a certain high continuous charge, for a certain sized termination. Working the machine so that the intensity should be just low enough to give continual brushes from the end in free air, the approach of a fine point changed these brushes into a glow. Working the machine so that the termination presented a continual glow in free air, the gradual approach of the hand caused the glow to contract at the very end of the wire, then to throw out a luminous point, which, becoming a foot stalk (1426.), finally produced brushes with large ramifications.

1541. Greasing the end of a rounded wire will immediately make it produce brushes instead of glow. A ball having a blunt point which can be made to project more or less beyond its surface, at pleasure, can be made to produce every gradation from glow, through brush, to spark.

1542. It is also very interesting and instructive to trace the transition from spark to glow, through the intermediate condition of stream, between ends in a vessel containing air more or less rarefied; but I fear to be prolix.

1543. All the effects show, that the glow is in its nature exactly the same as the luminous part of a brush or ramification, namely a charging of air; the only difference being, that the glow has a continuous appearance from the constant renewal of

the same action in the same place, whereas the ramification is due to a momentary, independent and intermitting action of the same kind.

Dark Discharge.

1544. I will now notice a very remarkable circumstance in the luminous discharge accompanied by negative glow, which may, perhaps, be correctly traced hereafter into discharges of much higher intensity. Two brass rods, 0·3 of an inch in diameter, entering a glass globe on opposite sides, had their ends brought into contact, and the air about them very much rarefied. A discharge of electricity from the machine was then made through them, and whilst that was continued the ends were separated from each other. At the moment of separation a continuous glow came over the end of the negative rod, the positive termination remaining quite dark. As the distance was increased, a purple stream or haze appeared on the end of the positive rod, and proceeded directly outwards towards the negative rod; elongating as the interval was enlarged, but never joining the negative glow, there being always a short dark space between. This space, of about $\frac{1}{14}$th or $\frac{1}{16}$th of an inch, was apparently invariable in its extent and its position, relative to the negative rod; nor did the negative glow vary. Whether the negative end were inductric or inducteous, the same effect was produced. It was strange to see the positive purple haze diminish or lengthen as the ends were separated, and yet this dark space and the negative glow remain unaltered (fig. 19.).

1545. Two balls were then used in a large air pump receiver, and the air rarefied. The usual transitions in the character of the discharge took place; but whenever the luminous stream, which appears after the spark and the brush have ceased, was itself changed into glow at the balls, the dark space occurred, and that whether the one or the other ball was made inductric, or positive, or negative.

1546. Sometimes when the negative ball was large, the machine in powerful action, and the rarefaction high, the ball would be covered over half its surface with glow, and then, upon a hasty observation, would seem to exhibit no dark space: but this was a deception, arising from the overlapping of the convex termination of the negative glow and the concave termination of the positive stream. More careful observation and experiment have convinced me, that when the negative glow occurs it never visibly touches the luminous part of the positive discharge, but that the dark space is always there.

1547. This singular separation of the positive and negative discharge, as far as concerns their luminous character, under circumstances which one would have thought very favourable to their coalescence, is probably connected with their differences when in the form of brush, and is perhaps even dependent on the same

cause. Further, there is every likelihood that the dark parts which occur in feeble sparks are also connected with these phenomena*. To understand them would be very important, for it is quite clear that in many of the experiments, indeed in all that I have quoted, discharge is taking place across the dark part of the dielectric to an extent quite equal to what occurs in the luminous part. This difference in the result would seem to imply a distinction in the modes by which the two electric forces are brought into equilibrium in the respective parts; and looking upon all the phenomena as giving additional proofs, that it is to the condition of the particles of the dielectric we must refer for the principles of induction and discharge, so it would be of great importance if we could know accurately in what the difference of action in the dark and the luminous parts consisted.

1548. The dark discharge through air (1552.), which in the case mentioned is very evident (1544.), leads to the inquiry, whether the particles of air are generally capable of effecting discharge from one to another without becoming luminous; and the inquiry is important, because it is connected with that degree of tension which is necessary to originate discharge (1368. 1370.). Discharge between *air and conductors* without luminous appearances are very common; and non-luminous discharges by carrying currents of air and other fluids (1562. 1595.) are also common enough: but these are not cases in point, for they are not discharges between insulating particles.

1549. An arrangement was made for discharge between two balls (1485.) fig. 15., but, in place of connecting the inducteous ball directly with the discharging train, it was put in communication with the inside coating of a Leyden jar, and the discharging train with the outside coating. Then working the machine, it was found that whenever sonorous and luminous discharge occurred at the balls A B, the jar became charged; but that when these did not occur, the jar acquired no charge: and such was the case when small rounded terminations were used in place of the balls, and also in whatever manner they were arranged. Under these circumstances, therefore, discharge even between the air and conductors was always luminous.

1550. But in other cases, the phenomena are such as to make it almost certain, that dark discharge can take place across air. If the rounded end of a metal rod, 0·15 of an inch in diameter, be made to give a good negative brush, the approach of a smaller end or a blunt point opposite to it will, at a certain distance, cause a diminution of the brush, and a glow will appear on the positive inducteous wire, accompanied by a current of air passing from it. Now, as the air is being charged both at the positive and negative surfaces, it seems a reasonable conclusion, that the charged portions meet somewhere in the interval, and there discharge to each other, without producing any luminous phenomena. It is possible, however, that the air electrified positively at the glowing end may travel on towards the negative surface, and actually form that atmosphere into which the visible negative brushes dart, in which case

* See Professor JOHNSON's experiments. SILLIMAN's Journal, xxv. p. 57.

dark discharge need not, of necessity, occur. But I incline to the former opinion, and think, that the diminution in size of the negative brush, as the positive glow comes on to the end of the opposed wire, is in favour of that view.

1551. Using rarefied air as the dielectric, it is very easy to obtain luminous phenomena as brushes, or glow, upon both conducting balls or terminations, whilst the interval is dark, and that, when the action is so momentary that I think we cannot refer to currents as affecting discharge across the dark part. Thus if two balls, about an inch in diameter, and 4 or more inches apart, have the air rarefied about them, and are then interposed in the course of discharge, an interrupted or spark current being produced at the machine*, each termination may be made to show luminous phenomena, whilst more or less of the interval is quite dark. The discharge will pass as suddenly as a retarded spark (295. 334.), i. e. in an interval of time almost inappreciably small, and in such a case, I think it must have passed across the dark part as true disruptive discharge, and not by convection.

1552. Hence I conclude that dark disruptive discharge may occur (1547. 1550.); and also, that, in the luminous brush, the visible ramifications may not show the full extent of the disruptive discharge (1444. 1452.), but that each may have a dark outside, enveloping, as it were, every part through which the discharge extends. It is probable, even, that there are such things as dark discharges analogous in form to the brush and the spark, but not luminous in any part (1445.).

1553. The occurrence of dark discharge in any case shows at how low a tension disruptive discharge may occur (1548.), and indicates that the light of the ultimate brush or spark is in no relation to the intensity required (1368. 1370.). So to speak, the discharge begins in darkness, and the light is a mere consequence of the quantity which, after discharge has commenced, flows to that spot and there finds its most facile passage (1418. 1435.). As an illustration of the growth generally of discharge, I may remark that, in the experiments on the transition in oxygen of the discharge from spark to brush (1518.), every spark was immediately preceded by a short brush.

1554. The phenomena relative to dark discharge in other gases, though differing in certain characters from those in air, confirm the conclusions drawn above. The two rounded terminations (1544.) (fig. 19.), were placed in *muriatic acid gas* (1445. 1463.) at the pressure of 6·5 inches of mercury, and a continuous machine current of electricity sent through the apparatus: bright sparks occurred until the interval was about or above an inch, when they were replaced by squat brushy intermitting glows upon both terminations, with a dark part between. When the current at the machine was in spark, then each spark caused a discharge across the muriatic acid gas, which, with a certain interval, was bright; with a larger interval, was straight across

* By spark current I mean one passing in a series of spark between the conductor of the machine and the apparatus: by a continuous current one that passes through metallic conductors, and in that respect without interruption at the same place.

and flamy, like a very exhausted and sudden, but not a dense sharp spark; and with a still larger interval, produced a feeble brush on the inductric positive end, and a glow on the inducteous negative end, the dark part being between (1544.); and at such times, the spark at the conductor, instead of being sudden and sonorous, was dull and quiet (334.).

1555. On introducing more muriatic acid gas, until the pressure was 29·97 inches, the same terminations gave bright sparks within at small distances; but when they were about an inch or more apart, the discharge was generally with very small brushes and glow, and frequently with no light at all, though electricity had passed through the gas. Whenever the bright spark did pass through the muriatic acid gas at this pressure, it was bright throughout, presenting no dark or dull space.

1556. In *coal gas*, at common pressures, when the distance was about an inch, the discharge was accompanied by short brushes on the ends, and a dark interval of half an inch or more between them, notwithstanding the discharge had the sharp quick sound of a dull spark, and could not have depended in the dark part on *convection*.

1557. This gas presents several curious points in relation to the bright and dark parts of spark discharge. When bright sparks passed between the rod ends 0·3 of an inch in diameter (1544.), very sudden dark parts would occur next to the brightest portions of the spark. Again, with these ends and also with balls (1422.), the bright sparks would be sometimes red, sometimes green, and occasionally green and red in different parts of the same spark. Again, in the experiments described (1518.), at certain intervals a very peculiar pale, dull, yet sudden discharge would pass, which, though apparently weak, was very direct in its course, and accompanied by a sharp snapping noise, as if quick in its occurrence.

1558. *Hydrogen* frequently gave peculiar sparks, one part being bright red, whilst the other was a dull pale gray, or else the whole spark was dull and peculiar.

1559. *Nitrogen* presented a very remarkable discharge, between two balls of the respective diameters of 0·15 and 2 inches (1506. 1518.), the smaller one being rendered negative either directly or inducteously. The peculiar discharge occurred at intervals between 0·42 and 0·68, and even at 1·4 inches when the large ball was inductric positively; it consisted of a little brushy part on the small negative ball, then a dark space, and lastly a dull straight line on the large positive ball (fig. 20.). The position of the dark space was very constant, and is probably in direct relation to the dark space described when negative glow was produced (1544.). When by any circumstance a bright spark was determined, the contrast with the peculiar spark described was very striking; for it always had a faint purple part, but the place of this part was constantly near the positive ball.

1560. Thus dark discharge appears to be decidedly established. But its establishment is accompanied by proofs that it occurs in different degrees and modes in different gases. Hence then another specific action, added to the many (1296. 1398. 1399. 1423. 1454. 1503.) by which the electrical relations of insulating dielectrics are distin-

guished and established, and another argument in favour of that molecular theory of induction, which is at present under examination*.

1561. What I have had to say regarding the disruptive discharge has extended to some length, but I hope will be excused in consequence of the importance of the subject. Before concluding my remarks, I will again intimate in the form of a query, whether we have not reason to consider the tension or retention and after discharge in air or other insulating dielectrics, as the same thing with retardation and discharge in a metal wire, differing only, but almost infinitely, in degree (1334. 1336.). In other words, can we not, by a gradual chain of association, carry up discharge from its occurrence in air, through spermaceti and water, to solutions, and then on to chlorides, oxides and metals, without any essential change in its character; and at the same time, connecting the insensible conduction of air, through muriatic acid gas and the dark discharge, with the better conduction of spermaceti, water, and the all but perfect conduction of the metals, associate the phenomena at both extremes? and may it not be, that the retardation and ignition of a wire are effects exactly correspondent in their nature to the retention of charge and spark in air? If so, here again the two extremes in property amongst dielectrics will be found to be in intimate relation, the whole difference probably depending upon the mode and degree in which their particles polarize under the influence of inductive actions (1338. 1603. 1610.).

¶ x. *Convection; or carrying discharge.*

1562. The last kind of discharge which I have to consider is that effected by the motion of charged particles from place to place. It is apparently very different in its nature to any of the former modes of discharge (1319.), but, as the result is the same, may be of great importance in illustrating, not merely the nature of discharge itself, but also of what we call the electric current. It often, as before observed, in cases of brush and glow (1440. 1535.), joins its effect to that of disruptive discharge, to complete the act of neutralization amongst the electric forces.

1563. The particles which being charged, then travel, may be either of insulating or conducting matter, large or small. The consideration in the first place of a large particle of conducting matter may perhaps help our conceptions.

1564. A copper boiler 3 feet in diameter was insulated and electrified, but so feebly, that dissipation by brushes or disruptive discharge did not occur at its edges or projecting parts in a sensible degree. A brass ball, 2 inches in diameter, suspended by

* I cannot resist referring here by a note to Biot's philosophical view of the nature of the light of the electric discharge, Annales de Chimie, liii. p. 321.

a clean white silk thread, was brought towards it, and it was found that, if the ball was held for a second or two near any part of the charged surface of the boiler, at such distance (two inches more or less) as not to receive any direct charge from it, it became itself charged, although insulated the whole time; and its electricity was the *reverse* of that of the boiler.

1565. This effect was the strongest opposite the edges and projecting parts of the boiler, and weaker opposite the sides, or those extended portions of the surface which, according to COULOMB's results, have the weakest charge. It was very strong opposite a rod projecting a little way from the boiler. It occurred when the copper was charged negatively as well as positively: it was produced also with small balls down to 0·2 of an inch and less in diameter, and also with smaller charged conductors than the copper. It is, indeed, hardly possible in some cases to carry an insulated ball within an inch or two of a charged plane or convex surface without its receiving a charge of the contrary kind to that of the surface.

1566. This effect is one of induction, not of communication. The ball, when related to the positive charged surface by the intervening dielectric, has its opposite sides brought into contrary states, that side towards the boiler being negative and the outer side positive. More inductric action is directed towards it than would have passed across the same place if the ball had not been there, for several reasons; amongst others, because, being a conductor, the resistance of the particles of the dielectric, which otherwise would have been there, is removed (1298.); and also, because the reacting positive surface of the ball being projected further out from the boiler than when there is no introduction of conducting matter, is more free therefore to act through the rest of the dielectric towards surrounding conductors, and so favours the exaltation of that inductric polarity which is directed in its course. It is, as to the exaltation of force upon its outer surface beyond that upon the inductric surface of the boiler, as if the latter were itself protuberant in that direction. Thus it acquires a state like, but higher than, that of the surface of the boiler which causes it; and sufficiently exalted to discharge at its positive surface to the air, or to affect small particles, as it is itself affected by the boiler, and they flying to it, take a charge and pass off; and so the ball, as a whole, is brought into the contrary inducteous state. The consequence is, that, if free to move, its tendency, under the influence of all the forces, to approach the boiler is increased, whilst it at the same time becomes more and more exalted in its condition, both of polarity and charge, until, at a certain distance, discharge takes place, it acquires the same state as the boiler, is repelled, and passing to that conductor most favourably circumstanced to discharge it, there resumes its first indifferent condition.

1567. It seems to me, that the manner in which inductric bodies affect uncharged floating or moveable conductors near them, is very frequently of this nature, and generally so when it ends in a carrying operation (1562. 1602.). The manner in which, whilst the dominant inductric body cannot give off its electricity to the air, the in-

ducteous body *can* effect the discharge of the same kind of force, is curious, and, in the case of elongated or irregularly shaped conductors, such as filaments or particles of dust, the effect will often be very ready, and the consequent attraction very immediate.

1568. The effect described is also probably influential in causing those variations in spark discharge referred to in the last series (1386. 1390.): for if a particle of dust were drawn towards the axis of induction between the balls, it would tend, whilst at some distance from that axis, to commence discharge at itself, in the manner described (1566.), and that commencement might so far facilitate the act (1417. 1420.) as to make the complete discharge, as spark, pass through the particle, though it might not be the shortest course from ball to ball. So also, with equal balls at equal distances, as in the experiments of comparison already described (1493. 1506.), a particle being between one pair of balls would cause discharge there in preference; or even if a particle were between each, difference of size or shape would give one for the time a predominance over the other.

1569. The power of particles of dust to carry off electricity in cases of high tension is well known, and I have already mentioned some instances of the kind in the use of the inductive apparatus (1201.). The general operation is very well shown by large light objects, as the toy called the electrical spider; or, if smaller ones are wanted for philosophical investigation, by the smoke of a glowing green wax taper, which, presenting a successive stream of such particles, makes their course visible.

1570. On using oil of turpentine as the dielectric, the action and course of small conducting carrying particles in it can be well observed. A few short pieces of thread will supply the place of carriers, and their progressive action is exceedingly interesting.

1571. A very striking effect was produced on oil of turpentine, which, whether it was due to the carrying power of the particles in it, or to any other action of them, is perhaps as yet doubtful. A portion of that fluid in a glass vessel had a large uninsulated silver dish at the bottom, and an electrified metal rod with a round termination dipping into it at the top. The insulation was very good, and the attraction and other phenomena striking. The rod end, with a drop of gum water attached to it, was then electrified in the fluid; the gum water soon spun off in fine threads, and was quickly dissipated through the oil of turpentine. By the time that four drops had in this way been commingled with a pint of the dielectric, the latter had lost by far the greatest portion of its insulating power; no sparks could be obtained in the fluid; and all the phenomena dependent upon insulation had sunk to a low degree. The fluid was very slightly turbid. Upon being filtered through paper only, it resumed its first clearness, and now insulated as well as before. The water, therefore, was merely diffused through the oil of turpentine, not combined with or dissolved in it: but whether the minute particles acted as carriers, or whether they were not rather gathered together in the line of highest inductive tension (1350.), and there,

being drawn into elongated forms by the electric forces, combined their effects to produce a band of matter having considerable conducting power, as compared with the oil of turpentine elsewhere, is as yet questionable.

1572. The analogy between the action of solid conducting carrying particles and that of the charged particles of fluid insulating substances, acting as dielectrics, is very evident and simple; but in the latter case the result is, necessarily, currents in the mobile media. Particles are brought by inductric action into a polar state; and the latter, after rising to a certain tension (1370.), is followed by the communication of a part of the force originally on the conductor; the particles consequently become charged, and then, under the joint influence of the repellent and attractive forces, are urged towards a discharging place, or to that spot where these inductric forces are most easily compensated by the contrary inducteous forces.

1573. Why a point should be so exceedingly favourable to the production of currents in a fluid insulating dielectric, as air, is very evident. It is at the extremity of the point that the intensity necessary to charge the air is first acquired (1374.); it is from thence that the charged particle recedes; and the mechanical force which it impresses on the air to form a current, is in every way favoured by the shape and position of the rod, of which the point forms the termination. At the same time, the point, having become the origin of an active mechanical force, does, by the very act of causing that force, namely, by discharge, prevent any other part of the rod from acquiring the same necessary condition, and so preserves and sustains its own predominance.

1574. The very varied and beautiful phenomena produced by sheltering or enclosing the point, illustrate the production of the current exceedingly well, and justify the same conclusions; it being remembered that in such cases the effect upon the discharge is of two kinds. For the current may be interfered with by stopping the access of fresh uncharged air, or retarding the removal of that which has been charged, as when a point is electrified in a tube of insulating matter closed at one extremity; or the *electric condition* of the point itself may be altered by the relation of other parts in its neighbourhood, also rendered electric, as when the point is in a metal tube, by the metal itself, or when it is in the glass tube, by a similar action of the charged parts of the glass, or even by the surrounding air which has been charged, and which cannot escape.

1575. Whenever it is intended to observe such inductive phenomena in a fluid dielectric as have a direct relation to, and dependence upon, the fluidity of the medium, such, for instance, as a discharge from points, or attractions and repulsions, &c., then the mass of the fluid should be great, and in such proportion to the distance between the inductric and inducteous surfaces as to include all the *lines of inductive force* (1369.) between them; otherwise, the effects of currents, attraction, &c., which are the resultants of all these forces, cannot be obtained. The phenomena which occur in the open air, or in the middle of a globe filled with oil of turpentine, will not take place

in the same media if confined in tubes of glass, shell-lac, sulphur, or other such substances, though they be excellent insulating dielectrics; nor can they be expected; for in such cases, the polar forces, instead of being all dispersed amongst fluid particles, which tend to move under their influence, are now associated in many parts with particles that, notwithstanding their tendency to motion, are constrained to remain quiescent.

1576. The varied circumstances under which, with conductors differently formed and constituted, currents can occur, all illustrate the same simplicity of production. A *ball*, if the intensity be raised sufficiently on its surface, and that intensity be greatest on a part consistent with the production of a current of air up to and off from it, will produce the effect like a point (1537.); such is the case whenever the glow occurs upon a ball, the current being essential to that phenomenon. If as large a sphere as can well be employed with the production of glow be used, the glow will appear at the place where the current leaves the ball, and that will be the part directly opposite to the connection of the ball and rod which supports it; but by increasing the tension elsewhere, so as to raise it above the tension upon that spot, which can easily be effected inductively, then the place of the glow and the direction of the current will also change, and pass to that spot which for the time is most favourable for their production (1591.).

1577. For instance, approaching the hand towards the ball will tend to cause brush (1539.), but by increasing the supply of electricity the condition of glow may be preserved; then on moving the hand about from side to side the position of the glow will very evidently move with it.

1578. A point brought towards a glowing ball would at twelve or fourteen inches distance make the glow break into brush, but when still nearer glow was reproduced, probably dependent upon the discharge of wind or air passing from the point to the ball, and this glow was very obedient to the motion of the point, following it in every direction.

1579. Even a current of wind could affect the place of the glow; for a varnished glass tube being directed sideways towards the ball, air was sometimes blown through it at the ball, and sometimes not. In the former case, the place of the glow was changed a little, as if it were blown away by the current, and this is just the result which might have been anticipated. All these effects illustrate beautifully the general causes and relations, both of the glow and the current of air accompanying it (1574).

1580. Flame facilitates the production of a current in the dielectric surrounding it. Thus, if a ball which would not occasion a current in the air have a flame, whether large or small, formed on its surface, the current is produced with the greatest ease; but not the least difficulty can occur in comprehending the effective action of the flame in this case, if its relation, as part of the surrounding dielectric, to the electrified ball, be but for a moment considered (1375. 1380.).

1581. Conducting fluid terminations, instead of rigid points, illustrate in a very beautiful manner the formation of the currents, with their effects and influence in exalting the conditions under which they were commenced. Let the rounded end of a brass rod, 0·3 of an inch or thereabouts in diameter, point downwards in free air; let it be amalgamated, and have a drop of mercury suspended from it; and then let it be powerfully electrized. The mercury will present the phenomenon of *glow*; a current of air will rush along the rod, and set off from the mercury directly downwards; and the form of the metallic drop will be slightly affected, the convexity at a small part near the middle and lower part becoming greater, whilst it diminishes all round at places a little removed from this spot. The change is from the form of *a* (fig. 21.) to that of *b*, and is due almost, if not entirely, to the mechanical force of the current of air sweeping over its surface.

1582. As a comparative observation, let it be noticed, that a ball gradually brought towards it converts the glow into brushes, and ultimately sparks pass from the most projecting part of the mercury. A point does the same, but at much smaller distances.

1583. Take next a drop of strong solution of muriate of lime; being electrified, a part will probably be dissipated, but a considerable portion, if the electricity be not too powerful, will remain, forming a conical drop (fig. 22.), accompanied by a strong current. If glow be produced, the drop will be smooth on the surface. If a short low brush is formed, a minute tremulous motion of the liquid will be visible; but both effects coincide with the principal one to be observed, namely, the regular and continuous charge of air, the formation of a wind or current, and the form given by that current to the fluid drop. If a discharge ball be gradually brought toward the cone, sparks will at last pass, and these will be from the apex of the cone to the approached ball, indicating a considerable degree of conducting power in this fluid.

1584. With a drop of water, the effects were of the same kind, and were best obtained when a portion of gum water or of syrup hung from a ball (fig. 23.). When the machine was worked slowly, a fine large quiet conical drop, with concave lateral outline, and a small rounded end, was produced, on which the glow appeared, whilst a steady wind issued, in a direction from the point of the cone, of sufficient force to depress the surface of uninsulated water held opposite to the termination. When the machine was worked more rapidly some of the water was driven off; the smaller pointed portion left was roughish on the surface, and the sound of successive brush discharges was heard. With still more electricity, more water was dispersed; that which remained was elongated and contracted, with an alternating motion; a stronger brush discharge was heard, and the vibrations of the water and the successive discharges of the individual brushes were simultaneous. When water from beneath was brought towards the drop, it did not indicate the same regular strong contracted current of air as before; and when the distance was such that sparks passed,

the water beneath was *attracted* rather than driven away, and the current of air *ceased*.

1585. When the discharging ball was brought near the drop in its first quiet glowing state (1582.), it converted that glow into brushes, and caused the vibrating motion of the drop. When still nearer, sparks passed, but they were always from the metal of the rod, over the surface of the water, to the point, and then across the air to the ball. This is a natural consequence of the deficient conducting power of the fluid (1584. 1585.).

1586. Why the drop vibrated, changing its form between the periods of discharging brushes, so as to be more or less acute at particular instants, to be most acute when the brush issued forth, and to be isochronous in its action, and how the quiet glowing liquid drop, on assuming the conical form, facilitated, as it were, the first action, are points, as to theory, so evident, that I will not stop to speak of them. The principal thing to observe at present is, the formation of the carrying current of air, and the manner in which it exhibits its existence and influence by giving form to the drop.

1587. That the drop, when of water, or a better conductor than water, is formed into a cone principally by the current of air, is shown amongst other ways (1594.) thus. A sharp point being held opposite the conical drop, the latter soon lost its pointed form; was retracted and became round; the current of air from it ceased, and was replaced by one from the point beneath, which, if the latter were held near enough to the drop, actually blew it aside, and rendered it concave in form.

1588. It is hardly necessary to say what happened with still worse conductors than water, as oil, or oil of turpentine; the fluid itself was then spun out into threads and carried off, not only because the air rushing over its surface, helped to sweep it away, but also because its insulating particles assumed the same charged state as the particles of air, and, not being able to discharge to them in a greater degree than the air particles themselves could do, were carried off by the same causes which urged these in their course. A similar effect with melted sealing-wax on a metal point forms an old and well-known experiment.

1589. A drop of gum water in the exhausted receiver of the air-pump was not sensibly affected in its form when electrified. When air was let in, it began to show change of shape when the pressure was ten inches of mercury. At the pressure of fourteen or fifteen inches the change was more sensible, and as the air increased in density the effects increased, until they were the same as those in the open atmosphere. The diminished effect in the rare air I refer to the relative diminished energy of its current; that diminution depending, in the first place, on the lower electric condition of the electrified ball in the rarefied medium, and in the next, on the attenuated condition of the dielectric, the cohesive force of water in relation to rarefied air being something like that of mercury to dense air (1581.), whilst that of water in dense air may be compared to that of mercury in oil of turpentine (1597.).

1590. When a ball is covered with a thick conducting fluid, as treacle or syrup, it is easy by inductive action to determine the wind from almost any part of it (1577.); the experiment, which before was of rather difficult performance, being rendered facile in consequence of the fluid enabling that part, which at first was feeble in its action, to rise into an exalted condition by assuming a pointed form.

1591. To produce the current, the electric intensity must rise and continue at *one spot*, namely, at the origin of the current, higher than elsewhere, and then, air having a uniform and ready access, the current is produced. If no current be allowed (1574.), then discharge may take place by brush or spark. But whether it be by brush or spark, or wind, it seems very probable that the initial intensity or tension at which a particle of a given gaseous dielectric charges, or commences discharge, is, under the conditions before expressed, always the same (1410.).

1592. It is not supposed that all the air which enters into motion is electrified; on the contrary, much that is not charged is carried on into the stream. The part which is really charged may be but a small proportion of that which is ultimately set in motion (1442.).

1593. When a drop of gum water (1584.) is made *negative*, it presents a larger cone than when made positive; less of the fluid is thrown off, and yet, when a ball is approached, sparks can hardly be obtained, so pointed is the cone, and so free the discharge. A point held opposite to it did not cause the retraction of the cone to such an extent as when it was positive. All the effects are so different from those presented by the positive cone, that I have no doubt such drops would present a very instructive method of investigating the difference of positive and negative discharge in air and other dielectrics (1480. 1501.).

1594. That I may not be misunderstood (1587.), I must observe here that I do not consider the cones produced as the result *only* of the current of air or other insulating dielectric over their surface. When the drop is of badly conducting matter, a part of the effect is due to the electrified state of the particles, and this part constitutes almost the whole when the matter is sealing-wax, oil of turpentine, and similar insulating bodies (1588.). But even when the drop is of good conducting matter, as water, solutions, or mercury, though the effect above spoken of will then be insensible 1607.), still it is not the mere current of air or other dielectric which produces all the change of form; for a part is due to those attractive forces by which the charged drop, if free to move, would travel along the line of strongest induction, and not being free to move, has its form elongated until the *sum* of the different forces tending to produce this form is balanced by the cohesive attraction of the fluid. The effect of the attractive forces are well shown when treacle, gum water, or syrup is used; for the long threads which spin out, at the same time that they form the axes of the currents of air, which may still be considered as determined at their points, are like flexible conductors, and show by their directions in what way the attractive forces draw them.

1595. When the phenomena of currents are observed in dense insulating dielectrics, they present us with extraordinary degrees of mechanical force. Thus, if a pint of well rectified and filtered (1571.) oil of turpentine be put into a glass vessel, and two wires be dipped into it in different places, one leading to the electrical machine, and the other to the discharging train, on working the machine the fluid will be thrown into violent motion throughout its whole mass, whilst at the same time it will rise two, three, or four inches up the machine wire, and dart off in jets from it into the air.

1596. If very clean uninsulated mercury be at the bottom of the fluid, and the wire from the machine be terminated either by a ball or a point, and also pass through a glass tube extending both above and below the surface of the oil of turpentine, the currents can be better observed, and will be seen to rush down the wire, proceeding directly from it towards the mercury, and there, diverging in all directions, will ripple its surface strongly, and mounting up at the sides of the vessel, will return to re-enter upon their course.

1597. A drop of mercury being suspended from an amalgamated brass ball, preserved its form almost unchanged in air (1581.); but when immersed in the oil of turpentine it became very pointed, and even particles of the metal could be spun out and carried off by the currents of the dielectric. The form of the liquid metal was just like that of the syrup in air (1584.), the point of the cone being quite as fine, though not so long. By bringing a sharp uninsulated point towards it, it could also be effected in the same manner as the syrup drop in air (1587.), though not so readily, because of the density and limited quantity of the dielectric.

1598. If the mercury at the bottom of the fluid be connected with the electrical machine, whilst a rod is held in the hand terminating in a ball three quarters of an inch, less or more, in diameter, and the ball be dipped into the electrified fluid, very striking appearances ensue. When the ball is raised again so as to be at a level nearly out of the fluid, large portions of the latter will seem to cling to it (fig. 24.). If it be raised higher, a column of the oil of turpentine will still connect it with that in the basin below (fig. 25.). If the machine be excited into more powerful action, this will become more bulky, and may then also be raised higher, assuming the form fig. 26; and all the time that these effects continue, currents and counter-currents, sometimes running very close together, may be observed in the raised column of fluid.

1599. It is very difficult to decide by sight the direction of the currents in such experiments as these. If particles of silk are introduced they cling about the conductors; but using drops of water and mercury the course of the fluid dielectric seems well indicated. Thus, if a drop of water be placed at the end of a rod (1571.) over the uninsulated mercury, it is soon swept away in particles streaming downwards towards the mercury. If another drop be placed on the mercury beneath the end of the rod, it is quickly dispersed in all directions in the form of streaming particles, the attractive forces drawing it into elongated portions, and the currents carrying them away

If a drop of mercury be hung from a ball used to raise a column of the fluid (1598.), then the shape of the drop seems to show currents travelling in the fluid in the direction indicated by the arrows (fig. 27.).

1600. A very remarkable effect is produced on these phenomena, connected with positive and negative charge and discharge, namely, that a ball charged positively raises a much higher and larger column of the oil of turpentine than when charged negatively. There can be no doubt that this is connected with the difference of positive and negative action already spoken of (1480. 1525.), and tends much to strengthen the idea that such difference is referable to the particles of the dielectric rather than to the charged conductors, and is dependent upon the mode in which these particles polarize (1503. 1523.).

1601. Whenever currents travel in insulating dielectrics they really effect discharge; and it is important to observe, though a very natural result, that it is indifferent which way the current or particles travel, as with reversed direction their state is reversed. The change is easily made, either in air or oil of turpentine, between two opposed and related rods, for an insulated ball being placed in connexion with either rod and brought near its extremity, will cause the current to set towards it from the opposite end.

1602. The two currents often occur at once, as when both terminations present brushes, and frequently when they exhibit the glow (1531.). In such cases, the charged particles, or many of them, meet and mutually discharge each other (1548. 1612.). If a smoking wax taper be held at the end of an insulating rod towards a charged prime conductor, it will very often happen that two currents will form, and be rendered visible by its vapour, one passing as a fine filament of smoky particles directly to the charged conductor, and the other passing as directly from the same taper wick outwards, and from the conductor; the principles of inductric action and charge, which were referred to in considering the relation of a carrier ball and a conductor (1566.), being here also called into play.

1603. The general analogy and, I think I may say, identity of action found to exist as to insulation and conduction (1338. 1561.) when bodies, the best and the worst in the classes of insulators or conductors, were compared, led me to believe that the phenomena of *convection* in badly conducting media were not without their parallel amongst the best conductors, such even as the metals. Upon consideration, the cones produced by Davy* in fluid metals, as mercury and tin, seemed to be cases in point, and probably also the elongation of the metallic medium through which a current of electricity was passing, described by Ampere†; for it is not difficult to conceive,

* Philosophical Transactions, 1823, p. 155. † Bibliotheque Universelle, xxi. 47.

that the diminution of convective effect, consequent upon the high conducting power of the metallic media used in these experiments, might be fully compensated for by the enormous quantity of electricity passing. In fact, it is impossible not to expect *some* effect, whether sensible or not, of the kind in question, when such a current is passing through a fluid offering a sensible resistance to the passage of the electricity, and, thereby, giving proof of a certain degree of insulating power (1328.).

1604. I endeavoured to connect the convective currents in air, oil of turpentine, &c. and those in metals, by intermediate cases, but found this not easy to do. On taking bodies, for instance, which, like water, acids, solutions, fused salts or chlorides, &c., have intermediate conducting powers, the minute quantity of electricity which the common machine can supply (371. 861.) is exhausted instantly, so that the cause of the phenomenon is kept either very low in intensity, or the instant of time during which the effect lasts is so small, that one cannot hope to observe the result sought for. If a voltaic battery be used, these bodies are all electrolytes, and the evolution of gas, or the production of other changes, interferes and prevents observation of the effect required.

1605. There are, nevertheless, some experiments which illustrate the connection. Two platina wires, forming the electrodes of a powerful voltaic battery, were placed side by side, near each other, in distilled water, hermetically sealed up in a strong glass tube, some minute filaments being present in the water. When, from the evolution of gas and the consequent increased pressure, the bubbles formed on the electrodes were so small as to produce but feebly ascending currents, then it could be observed that the filaments present were attracted and repelled between the two wires, as they would have been between two oppositely charged surfaces in air or oil of turpentine, moving so quickly as to displace and disturb the bubbles and the currents which these tended to form. Now I think it cannot be doubted that under similar circumstances, and with an abundant supply of electricity, of sufficient tension also, convective currents might have been formed; the attractions and repulsions of the filaments were, in fact, the elements of such currents (1572.), and therefore water, though almost infinitely above air or oil of turpentine as a conductor, is a medium in which similar currents can take place.

1606. I had an apparatus made (fig. 28.) in which *a* is a plate of shell-lac, *b* a fine platina wire passing through it, and having only the section of the wire exposed above; *c* a ring of bibulous paper resting on the shell-lac, and *d* distilled water retained by the paper in its place, and just sufficient in quantity to cover the end of the wire *b*; another wire, *e*, touched a piece of tin foil lying in the water, and was also connected with a discharging train; in this way it was easy, by rendering *b* either positive or negative, to send a current of electricity by its extremity into the fluid, and so away by the wire *e*.

1607. On connecting *b* with the conductor of a powerful electrical machine, not the least disturbance of the level of the fluid over the end of the wire during the

working of the machine could be observed; but at the same time there was not the smallest indication of electrical charge about the conductor of the machine, so complete was the discharge. I conclude that the quantity of electricity passed in a *given time* had been too small, when compared with the conducting power of the fluid, to produce the desired effect.

1608. I then charged a large Leyden battery (291.), and discharged it through the wire *b*, interposing, however, a wet thread, two feet long, to prevent a spark in the water, and to reduce what would else have been a sudden violent discharge into one of more moderate character, enduring for a sensible length of time (334.). I now did obtain a very brief elevation of the water over the end of the wire; and though a few minute bubbles of gas were at the same time formed there, so as to prevent me from asserting that the effect was unequivocally the same as that obtained by Davy in the metals, yet, according to my best judgement, it was partly, and I believe principally, of that nature.

1609. I employed a voltaic battery of 100 pair of four-inch plates for experiments of a similar nature with electrolytes. In these cases the shell-lac was cupped, and the wire *b* 0·2 of an inch in diameter. Sometimes I used a positive amalgamated zinc wire in contact with dilute sulphuric acid; at others, a negative copper wire in a solution of sulphate of copper; but, because of the evolution of gas, the precipitation of copper, &c., I was not able to obtain decided results. It is but right to mention, that when I made use of mercury, endeavouring to repeat Davy's experiment, the battery of 100 pair was not sufficient to produce the elevations*.

1610. The latter experiments (1609.) may therefore be considered as failing to give the hoped-for proof, but I have much confidence in the former (1605. 1608.), and in the considerations (1603.) connected with them. If I have rightly viewed them, and we may be allowed to relate the currents at points and surfaces in such extremely different bodies as air and the metals, and admit that they are effects of the *same* kind, differing only in degree and in proportion to the insulating or conducting power of the dielectric used, what great additional argument we obtain in favour of that theory, which in the phenomena of insulation and conduction also, as in these, would link *the same* apparently dissimilar substances together (1336. 1561.); and how completely the general view, which refers all the phenomena to the direct action of the molecules of matter, seems to embrace the various isolated phenomena as they successively come under consideration!

1611. The connection of this convective or carrying effect, which depends upon a certain degree of insulation, with conduction; i. e. the occurrence of both in so many

* In the experiments at the Royal Institution, Sir H. Davy used, I think, 500 or 600 pairs of plates. Those at the London Institution were made with the apparatus of Mr. Pepys, (consisting of an enormous single pair of plates), described in the Philosophical Transactions for 1823, p. 187.

of the substances referred to, as, for instance, the metals, water, air, &c., would lead to many very curious theoretical generalizations, which I must not indulge in here. One point, however, I shall venture to refer to. Conduction appears to be essentially an action of contiguous particles, and the considerations just stated, together with others formerly expressed (1326. 1336, &c.), lead to the conclusion, that all bodies conduct, and by the same process, air as well as metals; the only difference being in the necessary degree of force or tension between the particles which must exist before the act of conduction or transfer from one particle to another can take place.

1612. The question then arises, what is this limiting condition which separates, as it were, conduction and insulation from each other? Does it consist in a difference between the two contiguous particles, or the contiguous poles of these particles in the nature and amount of positive and negative force, no communication or discharge occurring unless that difference rises up to a certain degree, variable for different bodies, but always the same for the same body? Or is it true that, however small the difference between two such particles, if *time* be allowed, equalization of force will take place, even with the particles of such bodies as air, sulphur or lac? In the first case, insulating power in any particular body would be proportionate to the degree of the assumed necessary difference of force; in the second, to the *time* required to equalize equal degrees of difference in different bodies. With regard to airs, one is almost led to expect a permanent difference of force, but in all other bodies, time seems to be quite sufficient to ensure, ultimately, complete conduction. The difference in the modes by which insulation may be sustained, or conduction effected, is not a mere fanciful point, but one of great importance, as being essentially connected with the molecular theory of induction, and the manner in which the particles of bodies assume and retain their polarized state.

¶ xi. *Relation of a vacuum to electrical phenomena.*

1613. It would seem strange if a theory which refers all the phenomena of insulation and conduction, i. e. all electrical phenomena, to the action of contiguous particles, were to omit to notice the assumed possible case of a *vacuum*. Admitting that a vacuum can be produced, it would be a very curious matter indeed to know what its relation to electrical phenomena would be; and as shell-lac and metal are directly opposed to each other, whether a vacuum would be opposed to them both, and allow neither of induction or conduction across it. Mr. MORGAN* has said that a vacuum does not conduct. Sir H. DAVY concluded from his investigations, that as perfect a vacuum as could be made† did conduct, but does not consider the prepared spaces which he used as absolute vacua. In such experiments I think I have observed the

* Philosophical Transactions, 1785, p. 272. † Ibid. 1822, p. 64.

luminous discharge to be principally on the inner surface of the glass; and it does not appear at all unlikely, that, if the vacuum refused to conduct, still the surface of glass next it might carry on that action.

1614. At one time, when I thought inductive force was exerted in right lines, I hoped to illustrate this important question by making experiments on induction with metallic mirrors (used only as conducting vessels) exposed towards a very clear sky at night time, and of such concavity that nothing but the firmament could be visible from the lowest part of the concave *n*, fig. 29. Such mirrors, when electrified, as by connection with a Leyden jar, and examined by a carrier ball, readily gave electricity at the lowest part of their concavity if in a room; but I was in hopes of finding that, circumstanced as before stated, they would give little or none at the same spot, if the atmosphere above really terminated in a vacuum. I was disappointed in the conclusion, for I obtained as much electricity there as before; but on discovering the action of induction in curved lines (1231.), found a full and satisfactory explanation of the result.

1615. My theory, as far as I have ventured it, does not pretend to decide upon the consequences of a vacuum. It is not at present limited sufficiently, or rendered precise enough, either by experiments relating to spaces void of matter, or those of other kinds, to indicate what would happen in the vacuum case. I have only as yet endeavoured to establish, what all the facts seem to prove, that when electrical phenomena, as those of induction, conduction, insulation and discharge occur, they depend on, and are produced by the action of *contiguous* particles of matter, the next existing particle being considered as the contiguous one; and I have further assumed, that these particles are polarized; that each exhibits the two forces, or the force in two directions (1295. 1298.); and that they act at a distance only by acting on the *contiguous* and intermediate particles.

1616. But assuming that a perfect vacuum were to intervene in the course of the lines of inductive action (1304.), it does not follow from this theory, that the particles on opposite sides of such a vacuum could not act on each other. Suppose it possible for a positively electrified particle to be in the centre of a vacuum an inch in diameter, nothing in my present views forbids that the particle should act at the distance of half an inch on all the particles forming the inner superficies of the bounding sphere, and with a force consistent with the well-known law of the squares of the distance. But suppose the sphere of an inch were full of insulating matter, the electrified particle would not then, according to my notion, act directly on the distant particles, but on those in immediate association with it, employing *all* its power in polarizing them; producing in them negative force equal in amount to its own positive force and directed towards the latter, and positive force of equal amount directed outwards and acting in the same manner upon the layer of particles next in succession. So that ultimately, those particles in the surface of a sphere of half an inch radius, which were acted on *directly* when that sphere was a vacuum, will now be acted on *indirectly* as respects

the central particle or source of action, i. e. they will be polarized in the same way, and with the same amount of force.

§ 19. *Nature of the electric current.*

1617. The word *current* is so expressive in common language, that when applied in the consideration of electrical phenomena we can hardly divest it sufficiently of its meaning, or prevent our minds from being prejudiced by it (283. 511.). I shall use it in its common electrical sense, namely, to express generally a certain condition and relation of electrical forces supposed to be in progression.

1618. A current is produced both by excitement and discharge; and whatsoever the variation of the two general causes may be, the effect remains the same. Thus excitement may occur in many ways, as by friction, chemical action, influence of heat, change of condition, induction, &c.; and discharge has the forms of conduction, electrolyzation, disruptive discharge, and convection; yet the current connected with these actions, when it occurs, appears in all cases to be the same. This constancy in the character of the current, notwithstanding the particular and great variations which may be made in the mode of its occurrence, is exceedingly striking and important; and its investigation and development promise to supply the most open and advantageous road to a true and intimate understanding of the nature of electrical forces.

1619. As yet the phenomena of the current have presented nothing in opposition to the view I have taken of the nature of induction as an action of contiguous particles. I have endeavoured to divest myself of prejudices and to look for contradictions, but I have not perceived any in conductive, electrolytic, convective, or disruptive discharge.

1620. Looking at the current as a *cause*, it exerts very extraordinary and diverse powers, not only in its course and on the bodies in which it exists, but collaterally, as in inductive or magnetic phenomena.

1621. *Electrolytic action.*—One of its direct actions is the exertion of pure chemical force, this being a result which has now been examined to a considerable extent. The effect is found to be *constant* and *definite* for the quantity of electric force discharged (783, &c.); and beyond that, the *intensity* required is in relation to the intensity of the affinity or forces to be overcome (904. 906. 911.). The current and its consequences are here proportionate; the one may be employed to represent the other; no part of the effect of either is lost or gained; so that the case is a strict one, and yet it is the very case which most strikingly illustrates the doctrine that induction is an action of contiguous particles (1164. 1343.).

1622. The process of electrolytic discharge appears to me to be in close analogy, and perhaps in its nature identical with another process of discharge, which at first

seems very different from it, I mean *convection*. In the latter case the particles may travel for yards across a chamber; they may produce strong winds in the air, so as to move machinery; and in fluids, as oil of turpentine, may even shake the hand, and carry heavy metallic bodies about*; and yet I do not see that the force, either in kind or action, is at all different to that by which a particle of hydrogen leaves one particle of oxygen to go to another, or by which a particle of oxygen travels in the contrary direction.

1623. Travelling particles of the air can effect chemical changes just as well as the contact of a fixed platina electrode, or that of a combining electrode, or the ions of a decomposing electrolyte (453. 471.); and in the experiment formerly described, where eight places of decomposition were rendered active by one current (469.), and where charged particles of air in motion were the only electrical means of connecting these parts of the current, it seems to me that the action of the particles of the electrolyte and of the air were essentially the same. A particle of air was rendered positive; it travelled in a certain determinate direction, and coming to an electrolyte, communicated its powers; an equal amount of positive force was accordingly acquired by another particle (the hydrogen), and the latter, so charged, travelled as the former did, and in the same direction, until it came to another particle, and transferred its power and motion, making that other particle active. Now, though the particle of air travelled over a visible and occasionally a large space, whilst the particle of the electrolyte moved over an exceedingly small one; though the air particle might be oxygen, nitrogen, or hydrogen, receiving its charge from force of high intensity, whilst the electrolytic particle of hydrogen had a natural aptness to receive the positive condition with extreme facility; though the air particle might be charged with very little electricity at a very high intensity by one process, whilst the hydrogen particle might be charged with much electricity at a very low intensity by another process; these are not differences of kind, as relates to the final discharging action of these particles, but only of degree; not essential differences which make things unlike, but such differences as give to things, similar in their nature, that great variety which fits them for their office in the system of the universe.

1624. So when a particle of air, or of dust in it, electrified at a negative point, moves on through the influence of the inductive forces (1572.) to the next positive surface, and after discharge passes away, it seems to me to represent exactly that particle of oxygen which, having been rendered negative in the electrolyte, is urged by the same disposition of inductive forces, and going to the positive platina electrode, is there discharged, and then passes away, as the air or dust did before it.

* If a metallic vessel three or four inches deep, containing oil of turpentine, be insulated and electrified, and a rod with a ball (an inch or more in diameter) at the end have the ball immersed in the fluid whilst the end is held in the hand, the mechanical force generated when the ball is moved to and from the sides of the vessel will soon be evident to the experimenter.

1625. *Heat* is another direct effect of the *current* upon substances in which it occurs, and it becomes a very important question, as to the relation of the electric and heating forces, whether the latter is always definite in amount*. There are many cases, even amongst bodies which conduct without change, which stand out at present from the assumption that it is†; but there are also many which indicate that, when proper limitations are applied, the heat produced is definite. HARRIS has shown this for a given length of current in a metallic wire, using common electricity‡; and DE LA RIVE has proved the same point for voltaic electricity by his beautiful application of BREGUET's thermometer§.

1626. When the production of heat is observed in electrolytes under decomposition, the results are still more complicated. But important steps have been taken in the investigation of this branch of the subject by DE LA RIVE‖ and others; and it is more than probable that, when the right limitations are applied, constant and definite results will here also be obtained.

1627. It is a most important part of the character of the current, and essentially connected with its very nature, that it is always the same. The two forces are everywhere in it. There is never one current of force or one fluid only. Any one part of the current may, as respects the presence of the two forces there, be considered as precisely the same with any other part; and the numerous experiments which imply their possible separation, as well as the theoretical expressions which, being used daily, assume it, are, I think, in contradiction with facts (511, &c.). It appears to me to be as impossible to assume a current of positive or a current of negative force alone, or of the two at once with any predominance of one over the other, as it is to give an absolute charge to matter (1169. 1177.).

1628. The conviction of this truth, if, as I think, it be a truth, or on the other hand the disproof of it, is of the greatest consequence. If, as a first principle, we can establish that the centres of the two forces, or elements of force, never can be separated to any sensible distance, or at all events not further than the space between two contiguous particles (1615.), or if we can establish the contrary conclusion, how much more clear is our view of what lies before us, and how much less embarrassed the ground over which we have to pass in attaining to it, than if we remain halting between two opinions! And if, with that feeling, we rigidly test every experiment which bears upon the point, as far as our prejudices will let us (1161.), instead of permitting them with a theoretical expression to pass too easily away, are we not

* See DE LA RIVE's Researches, Bib. Universelle, 1829, xl. p. 40.

† Amongst others, DAVY, Philosophical Transactions, 1821, p. 438. PELLETIER's important results, Annales de Chimie, 1834, lvi. p. 371. and BECQUEREL's non-heating current, Bib. Universelle, 1835, lx. 218.

‡ Philosophical Transactions, 1824, pp. 225. 228.

§ Annales de Chimie, 1836, lxii. 177.

‖ Bib. Universelle, 1829, xl. 49; and RITCHIE, Phil. Trans. 1832, p. 296.

much more likely to attain the real truth, and from that proceed with safety to what is at present unknown?

1629. I say these things not, I hope, to advance a particular view, but to draw the strict attention of those who are able to investigate and judge of the matter, to what must be a turning point in the theory of electricity; to a separation of two roads, one only of which can be right: and I hope I may be allowed to go a little further into the facts which have driven me to the view I have just given.

1630. When a wire in the voltaic circuit is heated, the temperature frequently rises first, or most at one end. If this effect were due to any relation of positive or negative as respects the current, it would be exceedingly important. I therefore examined several such cases; but when, keeping the contacts of the wire and its position to neighbouring things unchanged, I altered the direction of the current, I found that the effect remained unaltered, showing that it depended, not upon the direction of the current, but on other circumstances. So there is here no evidence of a difference between one part of the circuit and another.

1631. The same point, i. e. uniformity in every part, may be illustrated by what may be considered as the inexhaustible nature of the current when producing particular effects; for these effects depend upon transfer only, and do not consume the power. Thus a current which will heat one inch of platina wire will heat a hundred inches (853. note). If a current be sustained in a constant state, it will decompose the fluid in one voltameter only, or in twenty others if they be placed in the circuit, in each to an amount equal to that in the single one.

1632. Again, in cases of disruptive discharge, as in the spark, there is frequently a dark part (1422.), which, by Professor JOHNSON, has been called the neutral point*; and this has given rise to the use of expressions implying that there are two electricities existing separately, which, passing to that spot, there combine and neutralize each other†. But if such expressions are understood as correctly indicating that positive electricity alone is moving between the positive ball and that spot, and negative electricity only between the negative ball and that spot, then what strange conditions these parts must be in; conditions, which to my mind are every way unlike that which really occurs! In such a case, one part of a current would consist of positive electricity only, and that moving in one direction; another part would consist of negative electricity only, and that moving in the other direction; and a third part would consist of an accumulation of the two electricities, not moving in either direction, but mixing up together, and being in a relation to each other utterly unlike any relation which could be supposed to exist in the two former portions of the discharge. This does not seem to me to be natural. In a current, whatever form the discharge may take, or whatever part of the circuit or current is referred to, as much positive force as is there exerted in one direction, so much negative force is there exerted in the other. If it were not so we should have bodies electrified not merely

* SILLIMAN's Journal, 1834, xxv. p. 57. † THOMSON on Heat and Electricity, p. 471.

positive and negative, but on occasions in a most extraordinary manner, one being charged with five, ten, or twenty times as much of both positive and negative electricity in equal quantities as another. At present, however, there is no known fact indicating such states.

1633. Even in cases of convection, or carrying discharge, the statement that the current is everywhere the same must in effect be true (1627.): for how, otherwise, could the results formerly described occur? When currents of air constituted the mode of discharge between the portions of paper moistened with iodide of potassium or sulphate of soda (465. 469.), decomposition occurred; and I have since ascertained that, whether a current of positive air issued from a spot, or one of negative air passed towards it, the effect of the evolution of iodine or of acid was the same, whilst the reversed currents produced alkali. So also in the magnetic experiments (307.) whether the discharge was effected by the introduction of a wire, or the occurrence of a spark, or the passage of convective currents either one way or the other, (depending on the electrified state of the particles) the result was the same, being in all cases dependent upon the perfect current.

1634. Hence, the section of a current compared with other sections of the same current must be a constant quantity, if the actions exerted be of the same kind; or if of different kinds, then the forms under which the effects are produced are equivalent to each other, and experimentally convertible at pleasure. It is in sections, therefore, we must look for identity of electrical force, even to the sections of sparks and carrying actions, as well as those of wires and electrolytes.

1635. In illustration of the utility and importance of establishing that which may be the true principle, I will refer to a few cases. The doctrine of unipolarity as formerly stated, and I think generally understood*, is evidently inconsistent with my view of a current (1627.); and the later singular phenomena of poles and flames described by Erman and others† partake of the same inconsistency of character. If a unipolar body could exist, i. e. one that could conduct the one electricity and not the other, what very new characters we should have a right to expect in the currents of single electricities passing through them, and how greatly ought they to differ, not only from the common current which is supposed to have both electricities travelling in opposite directions in equal amount at the same time, but also from each other! The facts, which are excellent, have, however, gradually been more correctly explained by Becquerel‡, Andrews§, and others; and I understand that Professor Ohms‖ has perfected the work, in his close examination of all the phenomena; and

* Erman, Annales de Chimie, 1807. lxi. p. 115. Davy's Elements, p. 168. Biot, Ency. Brit. Supp. iv. p. 444. Becquerel, Traité, i. p. 167. De la Rive, Bib. Univ. 1837. vii. 392.

† Erman, Annales de Chimie, 1824. xxv. 278. Becquerel, Ibid. xxxvi. p. 329.

‡ Becquerel, Annales de Chimie, 1831. xlvi. p. 283.

§ Andrews, Philosophical Magazine, 1836. ix. 182.

‖ Schweigger's Jahrbuch der Chemie, &c. 1830. Heft 8. Not understanding German, it is with extreme

after showing that similar phenomena can take place with good conductors, proves that with soap, &c. many of the effects are the mere consequences of the bodies evolved by electrolytic action.

1636. I conclude, therefore, that the *facts* upon which the doctrine of unipolarity was founded are not adverse to that unity and indivisibility of character which I have stated the current to possess, any more than the phenomena of the pile itself, which might well bear comparison with those of unipolar bodies, are opposed to it. Probably the effects which have been called effects of unipolarity, and the peculiar differences of the positive and negative surface when discharging into air, gases, or other dielectrics (1480. 1525.) which have been already referred to, may have considerable relation to each other*.

1637. M. DE LA RIVE has recently described a peculiar and remarkable effect of heat on a current when passing between electrodes and a fluid†. It is, that if platina electrodes dip into acidulated water no change is produced in the passing current by making the positive electrode hotter or colder; whereas making the negative electrode hotter increased the deflexion of a galvanometer affected by the current, from 12° to 30° and even 45°, whilst making it colder diminished the current in the same high proportions.

1638. That one electrode should have this striking relation to heat whilst the other remained absolutely without, seem to me as incompatible with what I conceived to be the character of a current as unipolarity (1627. 1635.), and it was therefore with some anxiety that I repeated the experiment. The electrodes which I used were of platina; the electrolyte, water containing about one sixth of sulphuric acid by weight: the voltaic battery consisted of two pairs of amalgamated zinc and platina plates in dilute sulphuric acid, and the galvanometer in the circuit was one with two needles, and gave when the arrangement was complete a deflexion of 10° or 12°.

1639. Under these circumstances heating either electrode increased the current; heating both produced still more effect. When both were heated, if either were cooled, the effect on the current fell in proportion. The proportion of effect due to heating this or that electrode varied, but on the whole heating the negative seemed to favour the passage of the current somewhat more than heating the positive.

regret I confess I have not access, and cannot do justice, to the many most valuable papers in experimental electricity published in that language. I take this opportunity also of stating another circumstance which occasions me great trouble, and, as I find by experience, may make me seemingly regardless of the labours of others:—it is a gradual loss of memory for some years past; and now, often when I read a memoir, I remember that I have seen it before, and would have rejoiced if at the right time I could have recollected and referred to it in the progress of my own papers.—M. F.

* See also HARE in SILLIMAN'S Journal, 1833. xxiv. 246.

† Bibliothèque Universelle, 1837. vii. 388.

Whether the application of heat were by a flame applied underneath, or one directed by a blow pipe from above, or by a hot iron or coal, the effect was the same.

1640. Having thus removed the difficulty out of the way of my views regarding a current, I did not pursue this curious experiment further. It is probable, that the difference between my results and those of M. DE LA RIVE may depend upon the relative values of the currents used; for I employed only a weak one resulting from two pairs of plates two inches long and half an inch wide, whilst M. DE LA RIVE used four pairs of plates of sixteen square inches in surface.

1641. Electric discharges in the atmosphere in the form of balls of fire have occasionally been described. Such phenomena appear to me to be incompatible with all that we know of electricity and its modes of discharge. As time is an element in the effect (1418. 1436.) it is possible perhaps that an electric discharge might really pass as a ball from place to place; but as every thing shows that its velocity must be almost infinite, and the time of its duration exceedingly small, it is impossible that the eye should perceive it as anything else than a line of light. That phenomena of balls of fire may appear in the atmosphere, I do not mean to deny; but that they have anything to do with the discharge of ordinary electricity, or are at all related to lightning or atmospheric electricity, is much more than doubtful.

1642. All these considerations, and many others, help to confirm the conclusion, drawn over and over again, that the current is an indivisible thing; an axis of power, in every part of which both electric forces are present in equal amount* (517. 1627.). With conduction and electrolyzation, and even discharge by spark, such a view will harmonize without hurting any of our preconceived notions; but as relates to convection, a more startling result appears, which must therefore be considered.

1643. If two balls A and B be electrified in opposite states and held within each other's influence, the moment they move towards each other, a current, or those effects which are understood by the word current, will be produced. Whether A move towards B, or B move in the opposite direction towards A, a current, and in both cases having the same *direction*, will result. If A and B move from each other, then a *current* in the opposite direction, or equivalent effects, will be produced.

1644. Or, as charge exists only by induction (1178. 1299.), and a body when electrified is necessarily in relation to other bodies in the opposite state; so, if a ball be electrified positively in the middle of a room and be then moved in any direction, effects will be produced, as if a *current* in the same direction (to use the conventional

* I am glad to refer here to the results obtained by Mr. CHRISTIE with magneto-electricity, Philosophical Transactions, 1833, p. 113. note. As regards the current in a wire, they confirm everything that I am contending for.

mode of expression) had existed: or, if the ball be negatively electrified, and then moved, effects as if a current in a direction contrary to that of the motion had been formed, will be produced.

1645. I am saying of a single particle or of two what I have before said, in effect, of many (1633.). If the former account of currents be true, then that just stated must be a necessary result. And, though the statement may seem startling at first, it is to be considered that, according to my theory of induction, the charged conductor or particle is related to the distant conductor in the opposite state, or that which terminates the extent of the induction, by all the intermediate particles (1165. 1295.), these becoming polarized exactly as the particles of a solid electrolyte do when interposed between the two electrodes. Hence the conclusion regarding the unity and identity of the current in the case of convection, jointly with the former cases, is not so strange as it might at first appear.

1646. There is a very remarkable phenomenon or effect of the electrolitic discharge, first pointed out, I believe, by Mr. Porrett, of the accumulation of fluid under decomposing action in the current on one side of an interposed diaphragm*. It is a mechanical result; and as the liquid passes from the positive towards the negative electrode in all the known cases, it seems to establish a relation to the polar condition of the dielectric in which the current exists (1164. 1525.). It has not as yet been sufficiently investigated by experiment; for De la Rive says†, it requires that the water should be a bad conductor, as, for instance, distilled water, the effect not happening with strong solutions; whereas, Dutrochet says‡ the contrary is the case, and that, the effect is not directly due to the electric current.

1647. Becquerel in his Traité de l'Electricité has brought together the considerations which arise for and against the opinion, that the effect generally is an electric effect§. Though I have no decisive fact to quote at present, I cannot refrain from venturing an opinion, that the effect is analogous both to combination and convection (1623.), being a case of carrying due to the relation of the diaphragm and the fluid in contact with it, through which the electric discharge is jointly effected; and further, that the peculiar relation of positive and negative small and large surfaces already referred to (1482. 1503. 1525.), may be the direct cause of the fluid and the diaphragm travelling in contrary but determinate directions. A very valuable experiment has been made by M. Becquerel with particles of clay‖, which will probably bear importantly on this point.

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* Annals of Philosophy, 1816. viii. p. 75.
† Annales de Chimie, 1835. xxviii. p. 196.
‡ Annales de Chimie, 1832. xlix. p. 423.
§ Vol. iv. p. 197. 192.
‖ Traité de l'Electricité, i. p. 285.

1648. *As long as* the terms *current* and *electro-dynamic* are used to express those relations of the electric forces in which progression of either fluids or effects are supposed to occur (283.), *so long* will the idea of velocity be associated with them; and this will, perhaps, be more especially the case if the hypothesis of a fluid or fluids be adopted.

1649. Hence has arisen the desire of estimating this velocity either directly or by some effect dependent on it; and amongst the endeavours to do this correctly, may be mentioned especially those of Dr. WATSON* in 1748, and of Professor WHEATSTONE† in 1834; the electricity in the early trials being supposed to travel from end to end of the arrangement, but in the later investigations a distinction occasionally appearing to be made between the transmission of the effect and of the supposed fluid by the motion of whose particles that effect is produced.

1650. Electrolytic action has a remarkable bearing upon this question of the velocity of the current, especially as connected with the theory of an electric fluid or fluids. In it there is an evident transfer of power with the transfer of each particle of the anion or cathion present, to the next particles of the cathion or anion; and as the amount of power is definite, we have in this way a means of localizing as it were the force, identifying it by the particle and dealing it out in successive portions, which leads, I think, to very striking results.

1651. Suppose, for instance, that water is undergoing decomposition by the powers of a voltaic battery. Each particle of hydrogen as it moves one way, or of oxygen as it moves in the other direction, will transfer a certain amount of electrical force associated with it in the form of chemical affinity (822. 852. 918.) onwards through a distance, which is equal to that through which the particle itself has moved. This transfer will be accompanied by a corresponding movement in the electrical forces throughout every part of the circuit formed (1627. 1634.), and its effects may be estimated, as, for instance, by the heating of a wire (853.) at any particular section of the current however distant. If the water be a cube of an inch in the side, the electrodes touching, each by a surface of one square inch, and being an inch apart, then, by the time that a tenth of it, or 25·25 grains, is decomposed, the particles of oxygen and hydrogen throughout the mass may be considered as having moved relatively to each other in opposite directions, to the amount of the tenth of an inch; i. e. that two particles at first in combination will after the motion be the tenth of an inch apart. Other motions which occur in the fluid will not at all interfere with this result; for they have no power of accelerating or retarding the electric discharge, and possess in fact no relation to it.

1652. The quantity of electricity in 25·25 grains of water is, according to an estimate of the force which I formerly made (861.), equal to above 24 millions of charges of a large Leyden battery; or it would have kept any length of a platina wire $\frac{1}{100}$ of an inch in diameter red hot for an hour and a half (853.). This result, though given

* Philosophical Transactions, 1748.

† Ibid. 1834, p. 583.

only as an approximation, I have seen no reason as yet to alter, and it is confirmed generally by the experiments and results of M. POUILLET*. According to Mr. WHEATSTONE's experiments the influence or effects of the current would appear at a distance of 576,000 miles in a second†. We have, therefore, in this view of the matter, on the one hand, an enormous quantity of power equal to a most destructive thunder storm appearing instantly at the distance of 576,000 miles from its source, and on the other, a quiet effect, in producing which the power had taken an hour and a half to travel through the tenth of an inch: yet these are the equivalents to each other, being effects observed at the sections of one and the same current (1634.).

1653. It is time that I should call attention to the lateral or transverse forces of the *current*. The great things which have been achieved by OERSTED, ARAGO, AMPERE, DAVY, DE LA RIVE, and others, and the high degree of simplification which has been introduced into their arrangement by the theory of AMPERE, have not only done their full service in advancing most rapidly this branch of knowledge, but have secured to it such attention that there is no necessity for urging on its pursuit. I refer of course to magnetic action and its relations; but though this is the only recognised lateral action of the current, there is great reason for believing that others exist and would by their discovery reward a close search for them (951.).

1654. The magnetic or transverse action of the current seems to be in a most extraordinary degree independent of those variations or modes of action which it presents directly in its course; it consequently is of the more value to us, as it gives us a higher relation of the power than any that might have varied with each mode of discharge. This discharge, whether it be by conduction through a wire with infinite velocity (1652.), or by electrolyzation with its corresponding and exceeding slow motion (1651.), or by spark, and probably even by convection, produces a transverse magnetic action always the same in kind and direction.

1655. It has been shown by several experimenters, that whilst the discharge is of the *same kind* the amount of lateral or magnetic force is very constant (366. 367. 368. 376.). But when we wish to compare discharge of different kinds, for the important purpose of ascertaining whether the same amount of current will in its *different forms* produce the same amount of transverse action, we find the data very imperfect. DAVY noticed, that when the electric current was passing through an aqueous solution it affected a magnetic needle‡, and Dr. RITCHIE says, that the current in the electrolyte is as magnetic as that in a metallic wire§, and has made water revolve round a magnet as a wire carrying the current would revolve.

1656. Disruptive discharge produces its magnetic effects: a strong spark, passed

* BECQUEREL, Traité de l'Electricité, v. p. 278.
† Philosophical Transactions, 1834, p. 589.
‡ Philosophical Transactions, 1821, p. 426.
§ Ibid. 1832, p. 294.

transversely to a steel needle, will magnetise it as well as if the electricity of the spark were conducted by a metallic wire occupying the line of discharge; and Sir H. DAVY has shown that the discharge of a voltaic battery in vacuo is affected and has motion given to it by approximated magnets*.

1657. Thus the three very different modes of discharge, namely, conduction, electrolyzation, and disruptive discharge, agree in producing the important transverse phenomenon of magnetism. Whether convection or carrying discharge will produce the same phenomenon has not been determined, and the few experiments I have as yet had time to make do not enable me to answer in the affirmative.

1658. Having arrived at this point in the consideration of the current and in the endeavour to apply its phenomena as tests of the truth or fallacy of the theory of induction which I have ventured to set forth, I am now very much tempted to indulge in a few speculations respecting its lateral action and its possible connexion with the transverse condition of the lines of ordinary induction (1165. 1304.). I have long sought and still seek for an effect or condition which shall be to statical electricity what magnetic force is to current electricity; for as the lines of discharge are associated with a certain transverse effect, so it appeared to me impossible but that the lines of tension or of inductive action, which of necessity precede that discharge, should also have their correspondent transverse condition or effect (951.).

1659. According to the beautiful theory of AMPERE, the transverse force of a current may be represented by its attraction for a similar current and its repulsion of a contrary current. May not then the equivalent transverse force of static electricity be represented by that lateral tension or repulsion which the lines of inductive action appear to possess (1304.)? Then again, when current or discharge occurs between two bodies, previously under inductrical relations to each other, the lines of inductive force will weaken and fade away, and, as their lateral repulsive tension diminishes, will contract and ultimately disappear in the line of discharge. May not this be an effect identical with the attractions of similar currents? i. e. may not the passage of static electricity into current electricity, and that of the lateral tension of the lines of inductive force into the lateral attraction of lines of similar discharge, have the same relation and dependencies, and run parallel to each other?

1660. The phenomena of induction amongst currents which I had the good fortune to discover some years ago (6. &c. 1048.) may perchance here form a connecting link in the series of effects. When a current is first formed, it tends to produce a current in the contrary direction in all the matter around it; and if that matter have conducting properties and be fitly circumstanced, such a current is produced. On the contrary, when the original current is stopped, one in the same direction tends to form all around it, and, in conducting matter properly arranged, will be excited.

* Philosophical Transactions, 1821. p. 427.

1661. Now though we perceive the effects only in that portion of matter which, being in the neighbourhood, has conducting properties, yet hypothetically it is probable, that the non-conducting matter has also its relations to, and is affected by, the disturbing cause, though we have not yet discovered them. Again and again the relation of conductors and non-conductors has been shown to be one not of opposition in kind, but only of degree (1334. 1603.); and, therefore, for this, as well as for other reasons, it is probable, that what will affect a conductor will affect an insulator also; producing perhaps what may deserve the term of the electrotonic state (60. 242. 1114).

1662. It is the feeling of the necessity of some lateral connexion between the lines of electric force (1114.); of some link in the chain of effects as yet unrecognised, that urges me to the expression of these speculations. The same feeling has led me to make many experiments on the introduction of insulating dielectrics having different inductive capacities (1270. 1277.) between magnetic poles and wires carrying currents, so as to pass across the lines of magnetic force. I have employed such bodies both at rest and in motion, without, as yet, being able to detect any influence produced by them; but I do by no means consider the experiments as sufficiently delicate, and intend, very shortly, to render them more decisive.

1663. I think the hypothetical question may at present be put thus: can such considerations as those already generally expressed (1658.) account for the transverse effects of electrical currents? are two such currents in relation to each other merely by the inductive condition of the particles of matter between them, or are they in relation by some higher quality and condition (1654.), which, acting at a distance and not by the intermediate particles, has, like the force of gravity, no relation to them?

1664. If the latter be the case, then, when electricity is acting upon and in matter, its direct and its transverse action are essentially different in their nature; for the former, if I am correct, will depend upon the contiguous particles, and the latter will not. As I have said before, this may be so, and I incline to that view at present, but I am desirous of suggesting considerations why it may not, that the question may be thoroughly sifted.

1665. The transverse power has a character of polarity impressed upon it. In the simplest forms it appears as attraction or repulsion, according as the currents are in the same or different directions: in the current and the magnet it takes up the condition of tangential forces; and in magnets and their particles produces poles. Since the experiments have been made which have persuaded me that the polar forces of electricity, as in induction and electrolytic action (1298. 1343.), show effects at a distance only by means of the polarized contiguous and intervening particles, I have been led to expect that *all polar forces* act in the same general manner; and the other kinds of phenomena which one can bring to bear upon the subject seem fitted to strengthen that expectation. Thus in crystallizations the effect is transmitted from

particle to particle; and in this manner, in acetic acid or freezing water a crystal a few inches or even a couple of feet in length will form in less than a second, but progressively and by a transmission of power from particle to particle. And, as far as I remember, no case of polar action, or partaking of polar action, except the one under discussion, can be found which does not act by contiguous particles*. It is apparently of the nature of polar forces that such should be the case, for the one force either finds or developes the contrary force near to it, and has, therefore, no occasion to seek for it at a distance.

1666. But leaving these hypothetical notions respecting the nature of the lateral action out of sight, and returning to the direct effects, I think that the phenomena examined and reasoning employed in this and the two preceding papers tend to confirm the view first taken (1164.), namely, that ordinary inductive action and the effects dependent upon it, are due to an action of the contiguous particles of the dielectric interposed between the charged surfaces or parts which constitute, as it were, the terminations of the effect. The great point of distinction and power (if it have any) in the theory is, the making the dielectric of essential and specific importance, instead of leaving it as it were a mere accidental circumstance or the simple representative of space, having no more influence over the phenomena than the space occupied by it. I have still certain other results and views respecting the nature of the electrical forces and excitation, which are connected with the present theory; and, unless upon further consideration they sink in my estimation, I shall very shortly put them into form as another series of these electrical researches.

* I mean by contiguous particles those which are next to each other, not that there is *no* space between them. See (1616.).

Royal Institution,
February 14th, 1838.

METEOROLOGICAL JOURNAL,

KEPT BY THE ASSISTANT SECRETARY,

AT THE APARTMENTS OF THE

ROYAL SOCIETY,

BY ORDER OF

THE PRESIDENT AND COUNCIL.

OBSERVANDA.

Height of the Cistern of the Barometer above the plinth at Waterloo Bridge....83 feet 2 inches.
_____ above the mean level of the sea .....97 feet.
Height of the receiver of the Rain Gauge above the court of Somerset House ..79 feet.
The External Thermometer is 2 feet higher than the Barometer Cistern.
The Thermometers are graduated to Fahrenheit's scale.
The Barometer is divided into inches and tenths.
The Hours of Observation are of Mean Time, the day beginning at Midnight.
The *daily* observations of the Barometer are *not* corrected.
The *monthly means* are corrected for capillarity and temperature by the Table contained in Mr. Baily's paper in *Phil. Trans.* for 1837.

METEOROLOGICAL JOURNAL FOR JULY AND AUGUST, 1837.

1837.		9 o'clock, A.M. Barometer uncorrected. Flint Glass.	9 o'clock, A.M. Barometer uncorrected. Crown Glass.	9 o'clock, A.M. Att. Ther.	3 o'clock, P.M. Barometer uncorrected. Flint Glass.	3 o'clock, P.M. Barometer uncorrected. Crown Glass.	3 o'clock, P.M. Att. Ther.	Dew Point at 9 o'clock, A.M.	Diff. of Wet and Dry Bulb Thermometer.	External Thermometers. Fahrenheit. 9 A.M.	3 P.M.	Self-registering. Lowest	Highest	Rain in Inches. Read off at 9 A.M.	Direction of the Wind at 9 A.M.	REMARKS.
JULY	S 1	30.336	30.328	62.8	30.316	30.312	63.3	50	06.7	56.7	62.3	50.0	75.3		E	A.M. Cloudy—light brisk wind. P.M. Fine—light clouds and wind. Evening, Fine and clear.
	● ☉ 2	30.332	30.326	65.2	30.240	30.236	63.5	52	07.1	58.0	69.7	47.5	65.2		ENE	Fine—light clouds and haze throughout the day. Evening, Cloudy.
	M 3	30.240	30.234	69.7	30.156	30.150	66.6	56	08.6	65.3	70.5	57.0	72.7		E	Fine—nearly cloudless—lt. haze throughout the day. Evening, Cldy.
	T 4	30.142	30.136	65.3	30.128	30.122	67.0	58	07.2	64.6	70.4	55.7	75.0		E	A.M. Cloudy—light breeze. P.M. Fine—light clouds. Evening, Cloudy.
	W 5	30.180	30.172	66.7	30.128	30.120	66.9	53	09.8	63.4	69.8	55.3	71.5		SSE	A.M. Cloudy—light breeze. P.M. Overcast. Evening, Light rain.
	T 6	30.150	30.144	67.8	30.136	30.130	68.3	60	08.7	65.6	70.3	60.6	72.3	.033	ESE	Overcast throughout the day.
	F 7	30.252	30.248	64.2	30.240	30.234	67.2	57	04.3	59.5	68.7	55.6	72.5		NE	A.M. Overcast—light wind. P.M. Fine—light clouds. Evening, Fine and clear.
	S 8	30.196	30.192	67.3	30.144	30.138	68.6	60	08.0	67.3	75.4	57.7	70.0		SW	Fine—nearly cloudless throughout the day. Evening, Overcast.
	☉ 9	30.164	30.156	66.0	30.154	30.150	65.8	50	07.2	57.9	60.7	51.6	76.4		E	A.M. Fine—light clouds—light brisk wind. P.M. Cloudy—light wind. Evening, Fine and clear.
	M10	29.970	29.964	65.7	29.926	29.920	64.9	54	06.9	61.4	69.8	50.0	63.9		E	A.M. Fine—light clouds and wind. P.M. Fine—nearly cloudless. Evening, Fine and clear.
	T 11	29.992	29.986	65.2	29.958	29.950	65.7	56	05.9	61.5	67.8	52.0	70.0		E	Fine—nearly cloudless—lt. breeze throughout the day. Ev. Cloudy.
	W12	29.958	29.950	61.7	29.916	29.908	63.6	54	04.3	57.7	63.5	54.8	70.2		E	Overcast—light wind throughout the day.
	T 13	29.818	29.812	62.5	29.804	29.796	63.9	56	05.9	62.5	65.8	56.9	64.2		S	Overcast—light breeze throughout the day.
	F 14	29.776	29.770	64.6	29.786	29.782	66.6	57	04.7	63.8	71.3	58.7	69.5		S	A.M. Overcast—brisk wind. P.M. Cloudy. 2½ P.M. Heavy shower. Evening, Overcast—occasional showers. 7 P.M. Rainbow visible.
	S 15	29.894	29.888	66.9	29.868	29.860	67.0	61	05.0	65.4	62.5	59.3	72.2	.250	S var.	A.M. Fine—dark threatening clouds—light wind. P.M. Overcast —heavy rain. Evening, Fine and clear.
	☉16	29.994	29.990	68.2	29.992	29.984	66.8	59	07.8	64.8	65.6	54.7	73.2	.355	SSW	A.M. Cloudy—light rain and wind. P.M. Overcast—light rain—distant thunder. Evening, Fine and clear.
	☽M17	30.044	30.040	69.8	29.994	29.990	67.5	58	08.3	64.5	68.5	53.5	72.2	.041	SW	A.M. Fine—lt. clouds & breeze. P.M. Cloudy. Ev. Ovcs.—lt. rain.
	T 18	29.866	29.860	65.2	29.828	29.820	67.2	60	05.8	64.3	69.5	58.7	70.8	.158	SW	A.M. Cloudy—light rain and wind. P.M. Cloudy—light breeze. Evening, Very light rain.
	W19	29.852	29.846	67.2	29.804	29.798	68.2	59	07.2	65.4	71.3	59.2	71.4		SSW	A.M. Cloudy—lt. wind. P.M. Fine—lt. clds. Ev. Overcast—lt. rain.
	T 20	29.900	29.896	68.0	29.922	29.916	67.8	60	07.8	64.8	63.3	58.0	72.0	.047	W	A.M. Cloudy—light breeze. P.M. Overcast—light rain. Evening, Continued rain.
	F 21	30.050	30.044	70.8	30.034	30.028	67.3	57	07.3	63.4	68.3	55.8	67.3	.102	WNW	Fine—light clouds and wind throughout the day. Evening, Cloudy —very light rain.
	S 22	30.088	30.082	66.0	30.058	30.052	67.3	57	06.2	63.3	71.7	56.5	69.8		NW	Fine—light clouds & breeze throughout the day. Ev. Fine & clear.
	☉23	30.058	30.052	68.7	30.000	29.996	70.0	62	06.9	68.4	76.5	59.9	72.2		SSW	A.M. Fine—lt. clouds. P.M. Cloudy. Ev. Overcast—very light rain.
	M24	30.146	30.142	74.2	30.126	30.120	71.4	57	07.7	63.7	71.2	59.0	78.3		NE	A.M. Fine—dark clouds—light breeze. P.M. Fine—light clouds. Evening, Cloudy.
	T25	30.126	30.120	71.0	30.108	30.102	71.0	60	08.8	67.0	72.3	61.3	72.3		W	Fine—light clouds & breeze throughout the day. Evening, Cloudy.
	W26	30.096	30.090	70.3	30.084	30.078	71.3	62	08.0	69.3	74.3	64.2	73.7		S	A.M. Fine—nearly cloudless. P.M. Cloudy. Evening, Fine & clear.
	T27	30.018	30.012	73.3	29.904	29.898	72.9	61	09.2	70.6	77.4	60.3	78.5		SSW	A.M. Fine—lt. clouds. P.M. Fine—nearly cloudless—stiff breeze. Evening, Cloudy.
	F 28	29.818	29.812	71.4	29.796	29.790	72.5	63	07.4	68.5	73.6	62.3	79.6		SW	A.M. Fine—lt. clds. & breeze. P.M. Cloudy—lt. rain. Ev. Overcast—light rain & wind.
	S 29	29.350	29.344	67.2	29.288	29.278	69.9	60	04.4	64.3	66.4	60.2	74.2	.255	SE var.	A.M. Ovc.—lt. rain, very h. wind. Sudden fall in Bar. 11 A.M. Heavy shr. P.M. Ovc.—occasional shrs. Ev. Ovc.—lt. rain—cont. h. wind.
	☉30	29.544	29.540	65.6	29.640	29.636	67.7	58	06.3	61.7	66.8	57.0	68.7	.091	SW	A.M. Overcast—wind abated, although very high during night. P.M. Cloudy. Ev. Overcast—occasional showers, with lightning.
	M31	29.924	29.922	69.6	29.890	29.886	67.4	56	07.2	62.8	68.6	53.7	68.3	.102	SW	A.M. Fine—lt. clouds. P.M. Cloudy, with occasional rain. Ev. Cldy.
	MEAN.	30.009	30.003	67.4	29.980	29.973	67.6	57.5	06.9	63.8	69.2	56.7	71.7	Sum. 1.434		Mean Barometer corrected ..... 9 A.M. F. 29.909 C. 29.909 .. 3 P.M. 29.979 29.971
AUGUST	● T 1	29.752	29.746	64.3	29.698	29.692	64.9	58	05.1	59.7	60.2	56.9	70.0	.036	SE	Overcast—light rain and wind throughout the day. Ev. High wind.
	W 2	29.694	29.688	64.7	29.672	29.666	67.4	60	04.2	64.5	68.2	57.4	64.8	.422	S var.	Overcast—light rain with high wind and partly off the day. Evening, Continued rain and wind.
	T 3	29.716	29.712	69.2	29.708	29.702	68.5	61	05.1	65.0	68.7	59.9	71.0	.033	S var.	A.M. Cloudy—light rain—high wind. P.M. Fine—light clouds—high wind. Evening, Overcast—light rain.
	F 4	29.896	29.892	68.2	29.948	29.944	68.0	60	07.0	64.3	68.3	57.2	69.5	.080	SW	A.M. Overcast—light rain and wind. P.M. Cloudy—4 P.M. heavy shower—6½ P.M. rainbow. Evening, Overcast, occasional shrs.
	S 5	30.182	30.176	67.5	30.194	30.186	66.3	56	08.8	61.3	65.3	52.2	69.3	.375	SW	Fine—light clouds and wind throughout the day. Ev. Fine & clear.
	☉ 6	30.266	30.258	66.8	30.250	30.246	66.0	55	06.9	60.7	66.0	52.0	66.0		E	Fine—light clouds and wind throughout the day. Evening, Cloudy.
	M 7	30.356	30.350	62.0	30.372	30.368	64.3	57	05.5	60.8	64.3	53.2	66.3		E	Overcast—light brisk wind throughout the day. Evening, Cloudy.
	T 8	30.396	30.392	65.5	30.338	30.332	64.5	55	08.0	61.7	65.8	51.5	65.6		NE	A.M. Cloudy—light brisk wind. P.M. Fine—light clouds & wind. Evening, Overcast.
	W 9	30.142	30.136	67.2	30.032	30.024	65.2	57	06.8	62.9	69.8	54.0	66.4		NE var.	A.M. Fine—light clouds—brisk wind. P.M. Cloudy—light wind.
	T10	29.968	29.962	63.3	29.916	29.910	66.2	59	04.6	62.0	72.0	57.3	70.2		E	A.M. Overcast—light breeze. P.M. Fine—light clouds and wind. Evening, Overcast—very light rain.
	F 11	29.884	29.878	65.8	29.916	29.908	67.8	61	05.7	65.2	71.4	61.3	72.0		NNW	A.M. Lightly Overcast. P.M. Fine—lt. clouds & wind. Ev. Overcast.
	S 12	30.016	30.010	68.0	29.994	29.988	68.6	61	06.1	65.3	72.5	60.3	72.0		SW	A.M. Lightly overcast. P.M. Fine—light clouds. Ev. Fine & clear.
	☉13	30.160	30.154	66.2	30.172	30.168	68.5	60	05.6	63.4	74.5	56.9	73.4		S	A.M. Overcast. P.M. Fine—light clouds. Evening, Cloudy.
	M14	30.286	30.282	70.3	30.258	30.252	70.2	61	07.5	68.3	75.4	59.6	74.6		E	Fine—nearly cloudless throughout the day.
	T15	30.278	30.274	71.0	30.210	30.204	70.6	60	07.2	65.0	73.3	56.5	77.0		E	A.M. Fine and clear—light breeze. P.M. Cloudy—light wind. Evening, Fine and clear.
	☽W16	30.140	30.132	67.3	30.068	30.064	69.7	61	04.8	65.2	69.4	58.5	75.0		E	Overcast—light rain with wind. Evening, Fine and clear.
	T17	30.044	30.038	68.5	30.010	30.006	71.5	66	02.8	67.5	75.8	62.5	74.0	.022	E	A.M. Overcast. P.M. Fine—light clouds. Evening, Fine and clear.
	F18	30.160	30.156	69.0	30.162	30.158	72.0	65	05.5	65.5	76.2	62.2	78.8	.008	SW	A.M. Overcast—light wind. P.M. Fine—light clouds.
	S 19	30.126	30.122	72.0	30.022	30.018	73.0	66	04.0	67.5	76.4	63.3	76.8		NNW	Overcast—light wind throughout the day. Evening, Fine—light clouds and wind.
	☉20	29.938	29.934	80.2	29.954	29.950	74.0	68	06.4	70.7	73.4	63.5	77.5		SW var.	A.M. Fine—light clouds and wind. P.M. Cloudy—brisk wind.
	M21	30.100	30.096	70.0	30.086	30.082	72.2	65	07.1	67.3	72.0	61.0	75.0		SW	Overcast—brisk wind throughout the day. Ev. Cloudy—light rain.
	T22	30.172	30.168	70.0	30.150	30.146	71.0	67	07.0	67.5	72.5	59.0	73.5		SSW	A.M. Cloudy—brisk wind. P.M. Overcast. Evening, Heavy shower.
	W23	30.054	30.050	69.2	30.042	30.038	71.0	65	04.0	65.8	71.0	65.2	73.6	.444	W	Overcast—light rain nearly the whole of the day. Ev. Heavy rain.
	T24	30.212	30.208	63.5	30.204	30.200	66.5	52	05.7	56.8	63.2	55.0	71.9	.638	N	A.M. Overcast—brisk wind. P.M. Fine—light clouds. Evening, Fine & clear.
	F25	30.228	30.224	62.0	30.148	30.146	64.0	57	05.0	58.5	66.0	50.4	63.3		E	A.M. Overcast. P.M. Fine—light clouds and wind.
	S 26	29.888	29.884	62.7	29.854	29.850	64.5	62	05.0	64.5	64.6	56.0	67.0	.022	SSW	A.M. Overcast—distant thunder and lightning—heavy rain. P.M. Overcast—very high wind.
	☉27	30.180	30.176	64.3	30.190	30.186	62.0	50	02.7	53.5	59.9	48.5	72.7	.763	E	Fine—light clouds—brisk wind nearly the whole of the day.
	M28	30.044	30.040	62.0	29.878	29.874	63.0	54	05.2	58.5	62.8	49.7	60.0		E	A.M. Cloudy. P.M. Fine—light clouds. Evening, Overcast.
	T29	29.504	29.500	60.0	29.460	29.456	61.0	54	00.8	55.5	59.4	53.2	65.5	.116	NE	Overcast—light rain throughout the day.
	W30	29.402	29.398	59.0	29.478	29.476	60.3	55	01.5	54.4	57.4	52.6	59.5	.602	NNW	Overcast, with occasional showers. Evening, Overcast—thunder and lightning.
	T31	29.456	29.450	60.8	29.414	29.408	60.5	54	02.4	56.4	62.2	49.2	59.3	.041	SSW	Fine—light clouds throughout the day. Evening, Fine and clear.
	MEAN.	30.021	30.016	66.5	29.993	29.989	67.2	59.7	05.7	62.7	68.3	56.6	70.0	Sum. 3.602		Mean Barometer corrected ..... 9 A.M. F. 29.993 C. 29.917 .. 3 P.M. 29.994 29.869

METEOROLOGICAL JOURNAL FOR SEPTEMBER AND OCTOBER, 1837.

1837.	9 o'clock, A.M. Barometer uncorrected. Flint Glass.	9 o'clock, A.M. Barometer uncorrected. Crown Glass.	9 o'clock, A.M. Att. Ther.	3 o'clock, P.M. Barometer uncorrected. Flint Glass.	3 o'clock, P.M. Barometer uncorrected. Crown Glass.	3 o'clock, P.M. Att. Ther.	Dew Point at 9 o'clock, A.M.	Difference between Dry Bulb and Thermometer.	External Thermometers. Fahrenheit. 9 A.M.	3 P.M.	Self-registering. Lowest.	Highest.	Rain at inches Read off at 9 A.M.	Direction of the Wind at 9 A.M.	REMARKS.
F 1	29.368	29.362	57.4	29.386	29.382	59.5	53	05.8	56.3	59.8	50.7	62.3		W	A.M. Overcast—light wind. P.M. Fine—light clouds and wind. Evening, Cloudy—light rain.
S 2	29.444	29.438	58.2	29.392	29.526	59.9	52	00.8	55.4	59.7	51.2	62.0	.091	N	A.M. Cloudy—light brisk wind. P.M. Fine—light clouds. Evening, Cloudy—light rain.
⊙ 3	29.666	29.660	59.8	29.704	29.700	59.0	49	05.1	53.4	55.8	50.3	61.2	.072	W	Overcast—light wind nearly the whole day. Evening, Light rain.
M 4	29.920	29.914	56.7	29.934	29.928	58.6	50	03.5	53.0	58.2	51.2	57.3	.055	WNW	Overcast—light wind throughout the day. Evening, Fine and clear.
T 5	29.864	29.808	57.3	29.912	29.906	58.6	52	04.0	56.6	61.7	49.3	59.7		E	A.M. Fine—light clouds and wind. P.M. Cloudy—light rain and wind. Evening, Cloudy.
W 6	30.060	30.052	55.3	30.010	30.002	57.4	50	03.7	52.0	60.7	47.6	61.6		N	A.M. Thick haze—light wind. P.M. Fine—nearly cloudless. Evening, Fine and clear.
T 7	29.946	29.942	58.8	29.872	29.866	60.2	52	04.4	59.3	63.3	51.3	61.6		S	A.M. Fine—light clouds—brisk wind. P.M. Overcast—brisk wind. Evening, Light rain.
F 8	29.930	29.924	59.6	29.932	29.926	60.5	54	02.5	55.5	61.9	54.5	66.0	.158	SE	A.M. Overcast—lt. rain. P.M. Fine—lt. clouds. Evening, Overcast.
S 9	29.872	29.866	61.7	29.734	29.730	62.5	55	05.6	60.8	66.8	55.5	62.2		SSE	A.M. Lightly overcast—light wind. P.M. Fine—light clouds—brisk wind. Evening, Overcast—light rain—high wind.
⊙ 10	29.916	29.910	64.4	29.940	29.936	64.0	56	03.7	60.2	67.2	54.7	67.3	.055	SSW	Fine—light clouds and wind throughout the day. Evening, Fine and clear.
M 11	29.822	29.818	64.0	29.718	29.714	64.0	59	04.0	62.5	64.4	56.2	68.0		SE var.	A.M. Fine—light clouds—brisk wind. P.M. Overcast—brisk wind. Evening, Heavy rain.
T 12	29.748	29.746	60.0	29.634	29.638	63.0	55	05.0	54.4	61.0	52.0	67.0	.277	SW	Overcast—light wind throughout the day. Evening, Fine and clear.
W 13	29.154	29.150	61.0	29.100	29.096	62.6	58	04.5	59.8	63.0	52.2	61.6	.052	S	A.M. Cloudy—light wind. P.M. Overcast—light wind.
○ T 14	29.354	29.350	58.0	29.468	29.464	61.0	53	04.5	56.0	59.2	53.0	64.0	.069	NNW	A.M. Overcast—light wind. P.M. Lightly cloudy—brisk wind.
F 15	29.634	29.630	59.0	29.750	29.746	58.6	52	04.5	55.5	58.4	49.8	59.5	.055	NW	A.M. Lightly overcast—light wind. P.M. Fine—light clouds and wind. Evening, Fine and clear.
S 16	30.004	30.000	56.0	29.964	29.960	57.4	52	05.0	55.0	60.6	48.5	60.0	.011	SE var.	Overcast—brisk wind throughout the day. Evening, Fine and clear—brisk wind.
⊙ 17	30.058	30.054	59.5	30.064	30.060	62.0	58	05.5	65.5	67.7	56.0	66.0		W var.	Lightly cloudy—brisk wind throughout the day. Evening, Overcast—high wind.
M 18	30.052	30.048	62.8	30.026	30.020	64.2	60	04.8	64.6	65.7	61.5	69.7		SSW	Overcast—brisk wind throughout the day.
T 19	30.170	30.164	63.2	30.122	30.116	65.0	60	05.9	63.4	67.8	58.8	68.3		SSE	Overcast—light wind throughout the day. Evening, Fine and clear.
W 20	30.028	30.022	65.6	29.948	29.942	65.6	61	06.0	66.3	65.2	61.2	68.5		E	Fine—nearly cloudless—lt. wind throughout the day. Ev. Fine & clear.
T 21	29.934	29.928	62.2	29.958	29.950	64.9	56	02.7	59.2	65.6	54.3	68.4		NE	A.M. Cloudy—light brisk wind. P.M. Fine—nearly cloudless—light wind. Evening, Fine and clear.
F 22	30.086	30.080	63.0	30.068	30.062	64.8	58	03.6	60.5	64.2	53.2	68.7		NE	A.M. Cloudy—light brisk wind. P.M. Fine—nearly cloudless—light wind. Evening, Fine and clear.
S 23	30.144	30.136	64.8	30.130	30.122	62.0	55	06.4	58.3	60.2	52.9	65.2		E	Fine and cloudless—light brisk wind nearly the whole of the day. Evening, Fine and clear.
⊙ 24	30.242	30.236	62.0	30.244	30.240	59.5	50	07.3	56.7	57.3	51.8	61.2		NE	Fine—lt. clouds—brisk wind throughout the day. Evening, Cloudy.
M 25	30.320	30.314	58.3	30.302	30.296	58.3	51	06.2	56.2	58.2	46.5	58.4		N	Fine—nearly cloudless—light wind. Evening, Fine and clear.
T 26	30.302	30.296	57.0	30.238	30.230	58.9	50	05.2	54.2	56.2	46.4	59.3		NE	A.M. Fine—nearly cloudless—light wind. P.M. Overcast—light rain. Evening, Cloudy.
W 27	30.116	30.110	54.9	30.074	30.066	57.2	49	03.1	51.2	57.6	44.4	60.2		N	Cloudy—light wind the whole of the day. Evening, Overcast.
T 28	30.076	30.072	55.3	30.040	30.034	58.5	50	04.2	53.4	59.3	44.4	60.2		N	A.M. Cloudy—light brisk wind. P.M. Fine—lt. clouds. Evening, Cloudy.
● F 29	30.064	30.058	55.8	30.008	30.004	56.8	50	03.9	54.9	58.7	44.4	60.2		NE	Fine—nearly cloudless—light wind. Evening, Cloudy.
S 30	29.972	29.968	55.8	29.946	29.942	57.9	51	02.9	57.3	61.7	48.0	59.6		NE	A.M. Fine—nearly cloudless. P.M. Fine—lt. clouds. Ev. Cloudy.
MEAN.	29.909	29.902	59.6	29.892	29.887	60.7	53.7	04.5	57.6	61.6	51.7	63.2	Sum. .895	Mean Barometer corrected	9 A.M. 29.830 .. 29.810 / 3 P.M. 29.822 .. 29.804

⊙ 1	29.928	29.924	57.8	29.930	29.926	61.3	53	04.2	57.4	62.2	48.2	62.6	.055	NE	Overcast—light fog throughout the day, with occasional rain.
M 2	30.158	30.152	60.7	30.176	30.170	62.3	58	04.2	62.2	66.5	48.0	63.8	.025	SSW	A.M. Lightly overcast—very light rain. P.M. Fine—light clouds. Evening, Fine and clear.
T 3	30.174	30.166	60.0	30.096	30.090	62.3	58	04.1	59.3	65.8	54.0	67.2		SE	A.M. Thick fog. P.M. Fine—light clouds and wind. Evening, Overcast—light rain.
W 4	30.048	30.042	62.2	30.080	30.074	64.4	58	05.0	63.2	65.4	54.0	67.3		SW	A.M. Lightly overcast. P.M. Fine—light clouds and wind. Evening, fog, Fine and clear.
T 5	30.264	30.256	61.2	30.246	30.240	62.8	57	04.8	57.4	61.7	52.7	68.5		WSW	A.M. Light fog. P.M. Cloudy—light wind. Evening, Overcast.
F 6	30.088	30.082	60.8	30.092	30.086	62.3	57	03.1	59.3	63.4	55.0	61.8	.361	SSW	A.M. Overcast—heavy rain. P.M. Fine—light clouds and wind. Evening, Fine and starlight.
S 7	30.246	30.240	59.4	30.228	30.220	60.8	53	01.3	53.7	62.0	49.2	63.4	.022	SW	A.M. Fine—very light fog and wind. P.M. Cloudy.
⊙ 8	30.204	30.198	59.0	30.126	30.122	60.0	54	04.6	58.3	60.0	52.7	62.0		SSW	Lightly overcast nearly the whole of the day, with light rain. Evening, Fine and starlight.
M 9	30.304	30.298	57.2	30.306	30.300	58.4	51	04.4	53.8	57.6	47.4	61.5	.033	W	Fine—light clouds throughout the day. Evening, Fine & starlight.
T 10	30.364	30.356	56.5	30.354	30.348	59.3	51	04.5	55.5	60.3	47.9	60.0		SW	A.M. Overcast—light wind. P.M. Fine—lt. clouds. Evening, Cloudy.
W 11	30.424	30.418	57.0	30.402	30.396	58.8	52	03.9	56.4	62.2	52.8	61.2		SW	A.M. Cloudy—light wind. P.M. Fine—lt. clouds. Ev. Fine & clear.
○ T 12	30.470	30.464	56.4	30.478	30.472	58.6	50	03.2	52.4	59.6	49.2	62.7		SW	A.M. Fine—nearly cloudless—very light fog. P.M. Cloudy—light wind. Evening, Fine & clear.
F 13	30.596	30.590	53.0	30.588	30.582	55.2	44	04.3	47.7	56.2	43.8	60.5		N	A.M. Fine & cloudless—light brisk wind. P.M. Cloudy—lt. wind.
S 14	30.708	30.700	52.4	30.672	30.664	53.4	45	03.5	47.2	54.3	43.9	56.5		N	Fine—nearly cloudless—light wind nearly the whole of the day. Evening, Foggy.
⊙ 15	30.640	30.634	50.8	30.562	30.558	52.0	44	02.6	44.2	54.3	38.5	55.6		W	A.M. Fine—very light fog. P.M. Fine—light clouds. Ev. Foggy.
M 16	30.488	30.480	50.6	30.402	30.392	52.4	44	04.3	47.7	54.7	44.0	54.6		SW	A.M. Light fog. P.M. Overcast. Evening, Fine and clear.
T 17	30.268	30.260	50.0	30.174	30.168	52.4	44	02.5	44.6	55.2	41.0	55.5		SSW	A.M. Light fog. P.M. Overcast. Evening, Very light rain.
W 18	30.112	30.104	52.0	30.120	30.112	53.7	46	03.8	52.0	57.8	44.3	56.2		SW	A.M. Fine—light clouds. P.M. Cloudy—lt. wind. Ev. Overcast.
T 19	30.400	30.396	51.3	30.408	30.402	53.2	44	02.6	45.2	55.2	44.0	58.4		S	A.M. Light fog. P.M. Fine—light clouds. Evening, Cloudy.
F 20	30.538	30.532	52.2	30.538	30.532	54.5	48	03.2	51.7	62.0	45.0	56.0		SW	A.M. Fine—light clouds—thick fog early. P.M. Cloudy.
S 21	30.640	30.632	53.8	30.588	30.580	54.9	49	03.8	52.4	55.7	50.9	62.3		N	A.M. Light fog. P.M. Overcast. Evening, Cloudy.
⊙ 22	30.456	30.450	53.2	30.362	30.358	55.0	47	03.0	49.4	57.8	47.0	56.2		SW	Cloudy—light fog and wind throughout the day. Evening, Overcast—light mist.
M 23	30.046	30.040	54.2	29.894	29.888	56.2	49	04.9	54.7	59.4	49.2	58.3		SSW	A.M. Cloudy—light wind. P.M. Fine—light clouds. Evening, Overcast—heavy rain.
T 24	29.594	29.590	54.7	29.494	29.490	54.8	49	02.2	50.4	50.2	50.2	60.2	.388	SSW	A.M. Overcast—heavy rain. P.M. Continued rain.
W 25	29.700	29.694	50.6	29.846	29.840	51.4	42	02.0	42.4	48.7	38.3	52.7	.250	N	Overcast—light rain—light brisk wind throughout the day.
T 26	30.062	30.054	47.6	29.964	29.956	52.3	40	02.8	42.3	51.6	35.4	49.4	.061	SW	A.M. Fine—lt. clouds & wind. P.M. Lightly overcast. Ev. Hi. wind.
F 27	29.486	29.480	51.4	29.540	29.534	52.2	48	02.0	53.4	51.4	42.0	54.2		S var.	A.M. Overcast—light rain—high wind—high wind during the night. P.M. Fine—light clouds and wind. Evening, Cloudy.
● S 28	29.610	29.602	48.8	29.428	29.420	50.3	42	03.9	47.3	49.7	40.8	54.0		S	A.M. Fine—light clouds and wind. P.M. Overcast—occasional rain. Evening, Continued rain.
⊙ 29	29.458	29.452	47.7	29.440	29.436	49.0	40	02.3	41.2	48.0	38.8	51.3	.477	SW	Fine—lt. clouds & wind during the day. Ev. Overcast—very lt. rain.
M 30	29.350	29.348	47.5	29.218	29.216	50.7	42	04.6	48.2	57.5	38.7	51.3	.041	S	Overcast—light brisk wind, with occasional rain throughout the day.
T 31	29.356	29.350	49.2	29.406	29.400	49.9	43	02.8	45.0	47.3	43.2	58.2	.108	SW	(Fine—nearly cloudless—light wind throughout the day. Evening, Overcast—high wind.
MEAN.	30.135	30.129	54.2	30.102	30.096	56.0	48.4	03.5	51.5	57.2	46.1	58.8	Sum. 1.821	Mean Barometer corrected	9 A.M. 30.070 .. 30.032 / 3 P.M. 30.063 .. 30.095

METEOROLOGICAL JOURNAL FOR NOVEMBER AND DECEMBER, 1837.

1837.	9 o'clock, A.M. Barometer uncorrected. Flint Glass.	9 o'clock, A.M. Barometer uncorrected. Crown Glass.	9 o'clock, A.M. Att. Ther.	3 o'clock, P.M. Barometer uncorrected. Flint Glass.	3 o'clock, P.M. Barometer uncorrected. Crown Glass.	3 o'clock, P.M. Att. Ther.	Dew Point at 9 A.M. from Attached Ther.	Diff. of Wet and Dry Bulb Ther.	External Thermometers. Fahrenheit. 9 A.M.	3 P.M.	Self-registering. Lowest	Highest	Rain in inches, from 9 A.M. to 9 A.M.	Direction of the Wind at 9 A.M.	REMARKS.
NOVEMBER															
W 1	28.978	28.974	48.8	28.780	28.774	51.3	45	02.7	52.4	55.8	41.4	54.3	.311	SSW	{Overcast—light rain—high wind throughout the day. Very high wind throughout the night.
T 2	29.130	29.126	48.4	29.166	29.160	49.4	42	03.0	42.8	45.6	41.0	56.4	.150	W	A.M. Overcast—lt. rain—h. wind. P.M. Cldy—lt. wind. Ev. Lt. rain.
F 3	29.258	29.252	45.3	29.320	29.314	47.0	37	02.3	39.7	45.8	36.4	48 8	.036	SW	{A.M. Cloudy—light fog and wind. P.M. Fine—light clouds. Evening, Light fog, with occasional flashes of lightening.
S 4	29.694	29.688	43.3	29.844	29.836	45.4	38	02.1	40.2	47.5	36.3	48.3		SW	{Fine—light clouds, with very light fog throughout the day. Evening, Fine and starlight.
⊙ S 5	30.072	30.064	42.8	30.074	30.068	45.0	36	02.4	39.8	47.4	36.8	48.3		WSW	A.M. Cloudy—light fog. P.M. Overcast. Evening, Light fog.
M 6	30.314	30.306	42.4	30.348	30.342	43.8	33	04.4	38.7	45.8	36.6	48.3		NW	{A.M. Fine—light fog and wind. P.M. Fine—light clouds and wind. Evening, Light fog.
T 7	30.456	30.450	41.4	30.406	30.400	42.6	33	01.8	34.7	44.8	32.8	46.8		E	A.M. Thick fog. P.M. Fine—nearly cloudless. Evening, Foggy.
W 8	30.320	30.312	39.3	30.246	30.238	39.7	32	01.5	32.0	38.6	30.4	45.3		E	A.M. Thick fog—white frost. P.M. Dense fog.
T 9	30.142	30.136	37.6	30.058	30.052	40.3	30	02.4	30.7	45.0	28.0	40.3		E	A.M. Light fog—sharp frost. P.M. Overcast—very light mist.
F 10	29.984	29.978	42.6	29.920	29.912	44.6	38	02.6	46.2	52.8	31.0	47.8	.019	WSW	Overcast—deposition nearly the whole day. Ev. Very light rain.
S 11	29.940	29.934	47.6	29.964	29.958	49.3	42	03.0	48.4	52.5	45.8	54.3	.050	WSW	Fine—light clouds and wind nearly the whole day. Ev. Overcast.
○ S 12	30.018	30.012	47.3	30.098	30.090	47.3	40	03.5	43.7	44.9	42.8	54.3		WSW	{A.M. Fine—nearly cloudless—light wind. P.M. Fine and cloudless. Evening, Fine and clear.
M 13	30.118	30.110	44.0	30.006	30.000	45.2	37	02.4	40.3	45.7	36.7	46.0		WSW	{A.M. Light fog and wind. P.M. Overcast—light rain. Evening, Heavy rain—high wind.
T 14	29.552	29.546	46.8	29.474	29.468	47.7	43	03.1	46.3	46.4	40.0	49.3	.286	W	A.M.—Nearly cloudless. P.M. Overcast—light rain.
W 15	29.948	29.942	43.8	30.018	30.010	44.5	37	02.7	38.6	42.3	36.8	48.7		N	Fine—light clouds & wind throughout the day. Ev. Fine—lt. clouds.
T 16	30.008	30.002	41.8	29.946	29.940	42.3	35	01.8	37.0	40.3	35.4	44.5		N	A.M. Light fog. P.M. Overcast. Evening, Light fog.
F 17	29.980	29.974	40.3	30.008	30.000	41.6	34	02.6	35.6	41.7	32.7	41.3		N	{Fine—light clouds and wind throughout the day—slight frost during the night. Evening, Fine and clear.
S 18	30.128	30.122	39.0	30.058	30.052	39.9	32	02.0	34.6	40.3	31.4	42.3		S	Light fog throughout the day—white frost during night. Ev. Overcast.
◑ S 19	30.010	30.004	41.9	29.900	29.896	44.6	37	03.2	44.8	51.5	33.2	45.5		SW	{A.M. Lightly overcast—deposition. P.M. Fine—light clouds. Evening, Overcast—heavy rain.
M 20	29.872	29.864	45.8	29.762	29.754	47.0	42	02.2	44.7	48.3	42.7	52.6	.200	SSW	{Fine—light clouds and wind nearly the whole of the day. Evening, Overcast—very light rain.
T 21	29.850	29.844	44.3	29.864	29.856	45.8	38	02.6	39.7	46.9	36.5	49.0		SSW	A.M. Fine—nearly cloudless. P.M. Fine—lt. clouds. Ev. Overcast.
W 22	29.930	29.926	45.7	29.900	29.896	47.9	42	02.7	50.6	54.7	39.8	51.3	.033	SW	{A.M. Overcast—very light rain and wind. P.M. Overcast—light wind. Evening, High wind.
T 23	29.806	29.800	50.6	29.714	29.708	51.6	48	03.1	52.4	53.7	50.3	55.2		S var.	Overcast—high wind throughout the day, and also night. Ev. Lt. rain.
F 24	29.954	29.948	51.2	29.960	29.954	51.3	46	02.0	46.7	48.6	46.4	55.0	.088	SW	{Light fog and wind during the whole of the day. Evening, Overcast—light rain.
S 25	30.092	30.086	47.3	30.156	30.150	47.3	40	02.4	39.3	42.4	39.2	49.0		N	Fine—light clouds & wind throughout the day. Ev. Fine & clear.
⊙ S 26	30.070	30.062	43.2	29.714	29.710	45.0	36	02 5	37.8	45.5	33.4	43.3		SW	{A.M. Lightly overcast—light wind. P.M. Overcast—light rain. Evening, Heavy rain.
M 27	29.524	29.518	43.2	29.494	29.488	44.2	37	01.9	37.8	43.3	37.2	48.7	.233	SW	{Fine—nearly cloudless, with light fog nearly the whole day. Evening, Overcast.
● T 28	29.282	29.276	43.7	29.214	29.208	44.5	39	02.3	42.2	43.0	37.9	45.0		WSW	A.M. Light fog and wind. P.M. Overcast—very light rain.
W 29	29.494	29.488	41.3	29.634	29.626	41.8	34	02.4	34.7	40.3	33.4	45.7	.013	W	{A.M. Fine—nearly cloudless—very light fog. P.M. Fine—light clouds. Evening, Fine and clear.
T 30	29.700	29.694	40.4	29.634	29.626	42.8	36	02.6	42.2	49.3	33.3	45.7		S	{A.M. Overcast—light mist. P.M. Cloudy. Evening, Overcast—light rain.
													Sum. 1.419		
MEAN.	29.854	29.848	44.0	29.823	29.816	45.4	38.0	02.5	41.2	46.4	37.2	49.0	Mean Barometer corrected		{ 9 A.M. 3 P.M. F. 29.817 .. 29.782 C. 29.809 .. 29.774
DECEMBER															
F 1	29.884	29.878	45.8	30.000	29.994	46,3	40	01.9	42.4	46.8	41.8	50.8	.038	SW	{A.M. Fine—nearly cloudless—very light fog. P.M. Fine—light clouds. Evening, Fine and starlight.
S 2	30.212	30.204	43.2	30.298	30.292	43.3	36	02.0	36.4	41.4	36.0	47.7		SW	Thick fog throughout the day.
⊙ S 3	30.428	30.420	41.4	30.412	30.408	42.0	35	01.4	37.2	40.2	35.7	42.6		ENE	Overcast—deposition—light wind throughout the day. Ev. Foggy.
M 4	30.500	30.492	39.4	30.484	30.476	39.7	31	01.6	33.2	38.5	32.4	41.3		N	A.M. Thick fog—white frost. P.M. Overcast. Evening, Light fog.
T 5	30.372	30.366	39.8	30.286	30.278	39.8	33	02.7	37.8	38.8	33.3	40.3		NE	A.M. Light fog and wind. P.M. Overcast.
W 6	30.126	30.120	39.7	29.996	29.992	39.6	33	02.7	37.9	36.4	36.8	40.0		N	{A.M. Light fog and wind. P.M. Overcast—light snow and wind. Evening, Light rain.
T 7	29.794	29.788	37.8	29.752	29.748	38.4	32	00.4	34.3	37.6	31.7	38.9	.061	NE	A.M. Overcast—light snow and wind. P.M. Overcast—light rain.
F 8	29.668	29.660	39.9	29.626	29.620	40.6	35	01.5	37.0	38.6	34.5	39.0	.075	NNE	Overcast throughout the day, with occasional rain.
S 9	29.690	29.686	39.3	29.732	29.726	39.6	34	01.7	35.4	37.8	34.7	41.0	.019	N	Overcast—light wind throughout the day. Evening, Very light mist.
○ M 10	30.024	30.016	39.3	30.016	30.012	40.0	34	01.2	36.2	40.2	34.6	40.2		N	Overcast—light wind throughout the day, with light rain.
M 11	30.110	30.104	39.2	30.092	30.086	39.9	33	03.0	37.9	38.8	36.0	41.5	.027	N	Overcast—light wind throughout the day. Evening, Light fog.
○ T 12	29.994	29.988	39.3	29.892	29.886	39.7	33	02.8	34.7	38.3	34.6	40.2		W	{A.M. Overcast—light fog and wind. P.M. Lightly overcast. Evening, Light fog and rain.
W 13	30.040	30.032	39.2	30.114	30.106	40.3	34	01.7	36.4	42.2	34.9	39.0		SW	A.M. Thick fog. P.M. Fine—lt. clouds. Ev. Light fog—deposition.
T 14	30.256	30.250	39.9	30.230	30.224	41.4	34	02.2	37.2	42.5	36.0	43.0		SE	A.M. Light fog. P.M. Fine—light clouds. Evening, Overcast.
F 15	30.156	30.150	41.0	30.064	30.058	41.3	37	02.3	37.7	38.8	36.6	43.4		SSE	Fine—light clouds and wind throughout the day. Ev. Overcast.
S 16	29.886	29.880	40.2	29.792	29.786	41.6	35	02.2	38.8	45.3	37.0	40.4		ESE	{A.M. Fine—nearly cloudless. P.M. Fine—light clouds. Evening, Overcast—light rain.
⊙ 17	29.808	29.802	44.0	29.720	29.716	46.0	40	01.4	46.8	49.5	38.7	48.2	.063	SE	Overcast—deposition the whole day. Ev. Heavy rain, with high wind.
M 18	29.362	29.354	47 3	29.416	29.410	49.0	46	01.9	50.5	51.7	46.5	51.4	.191	SW	{A.M. Heavy rain, with high wind. High wind throughout the night. P.M. Fine—light clouds. Evening, Light rain, with high wind.
T 19	29.976	29.970	49.0	29.928	29.922	49.6	44	01.7	45.2	50.5	45.2	53.6	.166	WSW	{A.M. Fine—light clouds. Very high wind the early part of the night. P.M. Overcast—light rain.
W 20	29.560	29.554	51.2	29.316	29.310	52,0	46	02.1	53.2	52.3	45.0	54.2	.286	SW	{A.M. Overcast—deposition—light wind. P.M. Light rain and snow. Evening, Very high wind.
T 21	29.964	29.956	48.3	30.160	30.152	47.7	42	02.6	42.2	43.0	42.0	55.2	.055	N	A.M. Cloudy—h. wind. P.M. Overcast—lt. brisk wind. Ev. High wind.
F 22	30.100	30.094	48.7	29.974	29.966	49.6	44	02.1	44.9	48.7	41.0	45.7		SSW	{A.M. Cloudy—light wind. P.M. Overcast—very light rain and wind. Evening, Overcast.
S 23	29.942	29.936	50.4	29.948	29.942	50.8	47	01.7	49.4	50.2	44.9	52.7		W	A.M. Light fog—deposition. P.M. Overcast. Evening, Overcast.
⊙ 24	30.020	30.012	49.5	29.890	29.886	50.0	45	02.4	47.3	52.5	46.0	50.8		SSW	A.M. Overcast—very light rain. P.M. Overcast—deposition.
M 25	29.796	29.790	51.0	29.868	29.864	52.0	47	02.8	52.2	54.4	47.0	53.3		SW var.	{A.M. Fine—heavy clouds—brisk wind. P.M. Fine—light clouds. Evening, Fine and starlight.
T 26	29.950	29.944	50.2	29.876	29.870	50.8	45	01.5	47.6	48.8	45.2	55.3		S	Overcast—deposition—light wind throughout the day.
● W 27	29.806	29.800	48.7	29.822	29.814	50.2	44	02.0	44.5	47.8	43.6	50.4		ENE	Overcast—light fog nearly the whole of the day.
T 28	29.896	29.892	49.4	29.902	29.896	50.3	45	02.2	48.8	49.7	44.3	49 7		S	A.M. Lightly overcast—light brisk wind. P.M. Overcast—light wind.
F 29	29.860	29.852	49.2	29.836	29.830	49.9	45	02.4	47.5	48.4	44 3	50.7		SE	{A.M. Fine—light clouds and wind. P.M. Fine—nearly cloudless. Evening, Overcast.
S 30	29.966	29.960	49.9	29.956	29.950	50.9	47	02.5	49 8	50.8	47.0	50.7		SE	{A.M. Overcast—light wind. P.M. Fine—light clouds and wind. Evening, Very light mist.
⊙ 31	29.994	29.988	49.6	29.996	29.990	51.0	45	02.1	46.7	48.7	46.3	51.8		SW	{A.M. Fine—light clouds. P.M. Lightly overcast. Evening, Fine and starlight.
													Sum. .981		
MEAN.	29.972	29.966	44.5	29.948	29.942	45.3	39.4	02.0	42.2	44.8	39.8	46.6	Mean Barometer corrected.....		{ 9 A.M. 3 P.M. F. 29.933 .. 29.907 C. 29.996 .. 29.900